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CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.
RICHARD TAYLOR, F.L.S. G.S. Astr. S. Nat. H. Mosc. &c.
AND
RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec nostros
vilius quia ex alienis libamus ut apes." *Just. Lips. Monit. Polit. lib. 1. cap. 1.*

VOL. XIII.

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ERRATA.

Page 45, Art. 34. line 8, *for directly read inversely.*
Page 395, line 11, *for precombined read uncombined.*

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1838.

I. *Education of Students in Civil Engineering and Mining in the University of Durham.**

IT has long been a subject of regret that no institution existed in England in which young men might receive an education which should peculiarly fit them for the higher branches of the profession of a Civil Engineer. That profession is comparatively of recent origin. It has been formed by the exigencies of the times, and has had to struggle with great disadvantages. The names of Smeaton, Brindley, and others recall to our minds the difficulties which those men of masterly abilities had to encounter, in order to devise the means and create the instruments necessary to accomplish the purposes which their genius conceived. A portion of this difficulty still subsists; while the attainments necessary to enable the engineer to meet the emergencies which he has to encounter, have continually become more varied and extensive.

The construction of canals, harbours and railroads, the successful application of steam to the purposes of navigation and of locomotive engines on land, the increased activity in opening out the treasures of coal and other minerals, in situations hitherto deemed inaccessible, have combined to offer a field of almost boundless extent for the exercise of talent of the highest order. In the meanwhile the profession of civil engineer has risen in the scale of national importance, in consequence of the immense capital employed under his direction. In no other profession, with the exception of that of the law, are so many questions of pecuniary importance

* Communicated by the Rev. Prof. Chevallier and Prof. Johnston.

submitted to the judgement of one man. In all public works unlimited confidence must be reposed in the skill and integrity of the civil engineer. If a company is formed, the individuals who compose it, and even the greater part of the directors, cannot be competent to form an opinion upon many of the questions which are of vital importance to the success of their undertaking. The peculiar obstacles to be encountered, the readiest and most efficacious means of overcoming them, or the most dexterous way to elude them, all require the union of long practice with natural talents and a cultivated mind. An error of judgement may entail a loss of millions of capital; and if such misconduct could be conceived possible, the want of integrity in the engineer would be ruinous to his employers.

The profession of civil engineer is also requiring from day to day a more extended range of information. Every part of mechanical science, as exercised in the construction of machinery, has received, and is still constantly receiving, great improvement. Questions arise respecting points which have only of late become a part of the civil engineer's practice. The relative position of the surface of very extended tracts of country, and the easiest lines of communication from place to place, require an extent of survey, which hitherto has been confined to the great geodetical operations undertaken for purposes purely scientific, accompanied with a minuteness of individual detail which even those purposes do not require. The extended processes of mining call for a knowledge of geology, mineralogy, chemistry, and metallurgy; and the constantly increasing boldness of speculation in undertakings of great extent, gives rise to practical problems of the greatest difficulty, and leads to the construction of works which will vie with the most magnificent structures of antiquity.

Yet, with all these increasing demands upon the skill and attainments of the civil engineers of this country, there has hitherto been found a great deficiency in the means of acquiring the requisite information. The education of young men at school has usually been too elementary to be of much service. The course of study pursued at the Universities has been too general and theoretical to be adapted to the particular wants of the young civil engineer; and although many valuable courses of lectures have been constantly given in the Universities on subjects intimately connected with the theory and practice of engineering and mining, those who are best acquainted with academical studies have been of opinion that the knowledge requisite for practical men would be more advantageously cultivated elsewhere. Hence those civil en-

gineers who have risen even to the highest rank in their profession have encountered the greatest difficulties in the early part of their career, for want of sufficient training in the course of their education. And although men of real talent and great perseverance have by the efforts of self-taught genius successfully mastered the obstacles thus unnecessarily thrown in their way, there can be little doubt that some even of these men would have received benefit from more systematic training, and that many others, who, under a proper course of instruction in their youth, would have attained eminence in their profession, have never risen above mediocrity.

The University of Durham is the first public body which has attempted to supply this deficiency in the system of education pursued in this country. It appeared to them that it was practicable to engraft the peculiar studies connected with civil engineering and mining upon the more general course of academical reading; and that, if such a union could be made, great benefit might be anticipated from the association of young men intended for the higher departments of civil engineering, with those who were destined for the learned professions, or for other stations in the higher or middle ranks of life. With this view, it was deemed desirable that the class of civil engineers should not form a separate body in the University, composing a college appropriated to themselves, but be admitted on the same footing as other students, subject to the same discipline, and engaged in a course of study which should be assimilated, as far as was practicable, to the general system of the University.

The following Regulations were accordingly prepared after much deliberation, and passed by the Senate and Convocation, in November 1837.

Regulations for Students in Civil Engineering, in the University of Durham, passed by the Senate and Convocation, Nov. 22, 1837.

1. Students shall be admissible, in conformity with the Regulations, Title ii., § 1, 2, 3, as Members of the University, subject to the ordinary University and College discipline, to go through a course of instruction in Civil Engineering.

2. No such Student shall be admitted, unless he has passed an Examination in the Latin language, in Arithmetic, and in the Elements of Mathematics.

This examination shall be conducted by two Examiners appointed by the Warden.

3. Every such Student shall be placed, like other Students, under a Tutor named by the Warden. The Tutor shall

direct his private studies, and shall have charge of his conduct and religious instruction.

4. The course of study for Engineer Students and the several lectures designed for them shall be under the immediate superintendence of the Professor of Mathematics, subject to the control of the Warden and Senate.

5. The full course of study shall extend over three years; and shall embrace the several subjects which relate to the theory and practice of Civil Engineering and Mining.

6. Engineer Students, who have completed their course, shall be admissible, by Grace of the University, to the Academical rank of Civil Engineer.

Engineer Students may, at an earlier period, receive Certificates of competency in the subjects in which they have been examined, as hereafter specified.

7. No Grace for admission to the rank of Civil Engineer shall be granted, unless the petitioner has passed three Public Examinations. The first of these shall be for Students who are in their third term of residence, at least; the second for such as have passed the first, and are in their sixth term of residence, at least; the third for such as have passed both the former, and are in their ninth term of residence, at least.

Yet any Engineer Student, who, at his admission to the University, shall pass the first Public Examination thus appointed for Engineer Students, shall be placed in the same position with regard to all terms and examinations relating to Engineer Students only, as if he had already kept three terms.

8. The first two of these Examinations shall be conducted by two or more Examiners nominated annually by the Warden, and approved by Convocation; and shall be directed to the subjects fixed by the Senate eleven months, at least, before.

After the second of these Examinations, any one who is specially recommended by the Examiners may obtain a formal Certificate from the Warden; this Certificate being limited to the particular subjects in which he has proved his competency.

9. The third and final Examination shall be conducted by three Examiners, at least, nominated by the Warden, and approved by Convocation; and shall be directed to the subjects fixed by the Senate eleven months, at least, before: provided always that every such Student shall then pass an examination in a modern language, or in some one Latin or Greek work, *melioris ævi et notæ*; the language or work to be selected by himself, but approved by the Senate.

10. All those who satisfy the Examiners, at each of these Examinations, shall be classed. The Senate shall have power to determine hereafter the subjects in which proficiency shall be deemed indispensable, and the nature of the classification.

11. Any student in arts, upon passing his first examination for the Degree of B.A., may proceed as an Engineer Student of the second year; and upon passing his second examination for the Degree of B.A., may proceed as an Engineer Student of the third year.

12. Any Engineer Student, who is recommended by his College, may petition the University, that terms which he has kept by residence as an Engineer Student may count towards the Degree of B.A.; and that the Examination passed by him in his third term of residence, at least, may be received instead of the first Examination for the Degree of B.A.; and the Examination passed by him in his sixth term of residence, at least, instead of the second Examination for the Degree of B.A.

13. The Warden and Senate shall have power to declare, in the course of Easter Term, 1838, that any Engineer Student, who shall have kept Epiphany Term, 1838, shall be regarded, with reference to all terms and examinations relating to Engineer Students only, as if he had kept also Michaelmas Term, 1837.

Orders made by the Senate, for carrying the above Regulations into effect.

1. The Course of Study for Engineer Students shall embrace the following subjects:—

Arithmetic.	Practical Mapping, and Architectural Drawing.
Algebra.	Theory of Perspective and Projections.
Euclid.	Hydrostatical and Hydraulical Instruments in general.
Logarithms.	The Steam Engine.
Plane Trigonometry.	Optical Instruments.
Solid Geometry.	Theoretical and Practical Chemistry.
Analytical Geometry.	Theory of Heat.
Theoretical and Practical Mechanics.	Mineralogy.
Differential and Integral Calculus.	Metallurgy.
Dynamics.	Geology.
Hydrostatics and Hydraulics.	The French, German, Spanish, and Italian Languages.
Pneumatics.	
Surveying, Levelling, use of Instruments.	

2. All Engineer Students who do not learn one of the

above-named Modern Languages shall attend Lectures in Latin or Greek during one term, at least, in each of their first two years of residence.

3. Besides keeping the ordinary Academical Terms, Engineer Students shall reside during the Easter vacation; and the Lectures shall be so arranged as to provide employment for them during their residence.

4. Engineer Students shall, during the first six terms of their residence as such, pay for tuition the sum of 10*l.* 10*s.* in each term; and during their further residence as such, the sum of 7*l.* in each term.

5. Every Student admitted to the Academical rank of Civil Engineer, shall receive a Certificate of such admission under the Common Seal of the University. The Certificate of competency given to Students after their second examination, (granted by the Warden, in conformity with section 8 of the Regulations of Nov. 22nd, 1837,) shall contain the names of the examiners on whose recommendation it is given.

6. The first two of the Public Examinations shall take place in the month of October, and the third and final Examination during the Easter Term, in each year.

7. Engineer Students shall pay a fee of 1*l.* on admission to each of the first two Public Examinations; a fee of 2*l.* on admission to the third and final Examination; and a fee of 3*l.* on admission to the rank of Civil Engineer, or on receiving a Certificate of competency.

8. Any Student in Arts may be admitted to attend any Course of Lectures designed for Engineer Students, upon the payment of such fee as the Senate shall hereafter direct.

9. Any Engineer Student may be admitted to attend any Course of Lectures designed for Students in Arts, upon such terms as the Senate shall hereafter direct.

The Regulations referred to in section 1, are as follows:—

Of Admission.

1. No one shall be held to be a Member of the University who has not been matriculated, that is, whose name has not been placed on the Register of the University by the authority of the Warden.

2. No Student shall be matriculated, unless he is a Member of the existing College, or of some other recognised College, Hall, or House, nor unless he has produced to the Warden satisfactory testimonials of character.

3. Every Student, at the time of his matriculation, shall subscribe a declaration of obedience to the Authorities of the University.

As this is the first plan of the kind which has been attempted by Google

in this country, and many of the details may be interesting to some who are not familiar with academical studies, it may be desirable to state somewhat more at length the nature of the studies pursued.

The students at the time of their admission are required to be well acquainted with arithmetic, both as applied to commercial purposes and as used in the ordinary computations of engineering. They pass an examination also in the Latin language and in the elements of mathematics. If, on admission, a student is sufficiently advanced to pass the examination appointed for those engineer students who have already resided for a year, he is entitled to proceed at once in the course of reading intended for those who are then commencing their second year's residence, and to receive his final certificate at the end of two years' residence instead of three.

The lectures for the different years are arranged according to the degree of proficiency which the students have attained, beginning with geometry and the elementary parts of mathematics; arithmetic, especially as relating to the course of exchange, vulgar and decimal fractions and the extraction of roots; and proceeding in the course of subjects pointed out in the regulations. The modern languages which are generally taught are French and German. During their whole course the students are engaged in practical surveying, leveling, and planning; and in acquiring the familiar use of instruments, under the superintendence of an experienced civil engineer; and they receive instruction in crystallography, mineralogy, geology, and metallurgy. The local position of the city of Durham, in the immediate neighbourhood of the largest coal-mines in the world, at no great distance from the lead-mines of the western part of the county, and in a district intersected in all directions by railroads carried through very difficult lines of country, gives peculiar advantages for acquiring a practical insight into all the details of the ordinary operations of civil engineering and mining; and full use is made of these facilities.

The proficiency of the students is tested by public examinations every year. At the close of their three years' course, honours are given to those who distinguish themselves; and those who have passed all the examinations are admitted to the academical rank of civil engineer, and receive certificates to that effect. Certificates of competency in any particular branch of study may be received at an earlier period.

It is hoped that the course of instruction thus pursued will secure for the student a sound knowledge of those parts of theoretical mathematics and of the sciences of observation, which are essential for the scientific engineer, and at the same

time give him a perfect acquaintance with all the necessary practical details.

It has already been observed that the engineer students are subject to the same discipline and moral control as the other students in the University, an advantage which will be duly appreciated by those parents and others who are desirous of securing for a young man the advantage of a sound education, under wholesome control, and in the society of young men of the same age intended for other professions.

By the regulations provision is made for any engineer student to proceed to the degree of bachelor of arts, on passing the requisite examinations for that purpose. There are several scholarships in the University, to which engineer students, as well as others, are eligible.

The class of engineers was opened in January 1838, and at present consists of eight students. The academical year, consisting of three terms, begins in October; and the lectures are arranged so that the course of instruction commences at that time.

The fees payable by engineer students for tuition are 10*l.* 10*s.* in each term. This includes all payments to the professors and other officers of the University, and gives the student admission to all the lectures given to his class. The students have rooms in the University College, and dine in the college hall. It is understood that the whole expense of residence during the academical year, including tuition, certainly need not exceed 100*l.*

It will be borne in mind that the new course of study established by the University of Durham is such as to form not merely a *school of civil engineers* in the ordinary sense of the term, but also a *School of Mines*, in which persons likely to be through life engaged either in excavating the mineral wealth of various kinds with which the country abounds, or in converting the raw mineral into an article of commerce, may receive the elements of the several branches of knowledge which their pursuits may require. For the especial benefit of the latter class of persons it is provided that at a certain period in the course of study a more undivided attention shall be given to the theory and processes of metallurgy in its various branches than is considered at all necessary for the engineer students as a body. The importance of this regulation will be understood by those who are aware of the many *desiderata* and unexplained circumstances which the smelter of iron, lead, tin, or copper can detail, or of the national waste of material with which even the most improved processes are attended*.

* Our readers will remember that the importance of establishing a School of Mines in this country has long been urged by Mr. John Taylor, Treas.

II. *On the Radii and Distance of the Primary and Secondary Rainbows, as found by Observation, and on a Comparison of their Values with those given by Theory.* By R. POTTER, Esq., M.A.*

THE explanation of the rainbow which Sir Isaac Newton has given in the second part of 'the first book of Opticks,' appeared one of the happiest applications of his great optical discovery, the unequal refrangibility of light. It has, however, been long observed that there are frequently supernumerary bows attending the principal ones, and of these the theory of Sir Isaac Newton gives no solution. It was reserved for Dr. Young to show that they were results of the principle of interferences. (See his Lectures on Natural Philosophy, or Phil. Trans. for 1803.) But notwithstanding the satisfactory manner in which all the phænomena of the rainbow appeared to be accounted for, when the interference of the light was taken into consideration, yet the problem continued to be discussed in our optical treatises according to the old method, and Dr. Young's theory, like most of his other fine discoveries, did not receive the notice it deserved.

In the year 1835, I re-discovered that the solution of these phænomena involved the principle of interferences, without knowing what Dr. Young had written, and presented a paper to the Cambridge Philosophical Society, which is printed in the sixth volume of the Transactions, in which I undertook to show that the problem belonged to physical optics. Near the close of that paper will be found an intimation that the ordinary rainbows might not eventually be found in the positions which had hitherto been assigned to them, by Dr. Young as well as by all others.

My views with respect to interference on the corpuscular theory of light (see Lond. and Edinb. Phil. Mag. vol. ii. p. 81. and 'Correspondance Mathématique et Physique de l'Observatoire de Bruxelles,' 2^e livraison, tome 8.) had led me to expect this, and I had, at the time of writing that paper, examined Sir Isaac Newton's discussion sufficiently to convince myself, that there were abundant grounds for anticipating a full confirmation when I had time to pursue the investigation more completely.

In the beginning of the year 1836, I compared the mea-

Geol. Soc. whose Prospectus on the subject will be found in Phil. Mag., First Series, vol. lxi. p. 137, and in his "Records of Mining." Mr. H. English has also repeatedly brought the same subject before the public in his Mining Review.—EDIT.

* Communicated by the Author.

tures of the radii of the primary and secondary bows and their distance, as given by Sir Isaac Newton, with the results obtained from the theory, by using Fraunhofer's correct refractive indices. This comparison, which is detailed below, completely establishes the discovery that the ordinary rain-bows are not in the places hitherto assigned to them.

I had confirmed Sir Isaac Newton's measures in the year 1834, by measures of the radii of the iris seen frequently in the mornings of autumn, in the dew-drops which float on the scum of stagnant ponds. These measures, although they had no claims to very great accuracy, were yet so nearly in accordance with those of Sir Isaac Newton as to convince me that the wide discrepancies between the theoretical and observed radii could not possibly arise from errors of observation; and this I urged in a discussion at a meeting of the Cambridge Philosophical Society in the spring of 1836. Since then, the Astronomer Royal has deduced expressions on the undulatory theory of light, (see the May number of this journal, p. 452,) which indicate that the *brightest parts* of the bow ought to have their position different from those calculated from previous theories, and in the direction which the observed measures require; but he finds that the light shading away from the maximum of brightness, has at the old position a brightness which is about one half of that at the maximum, and which ought therefore to be distinctly seen: this is the point to be examined in order to test his expressions.

The general discovery that the bows are not in the positions assigned to them hitherto, has been confirmed by measures taken by Professor Miller in M. Babinet's experiment, in which the phænomena analogous to those forming the rain-bows are viewed in a small cylindrical stream of water.

Proceeding to discuss the observations, we find that Sir Isaac Newton's principal measurements were on the *extreme radius* of the primary bow and on the *least* distance between the two bows; for after giving the results of his computations he says, "And such are the dimensions of the bows in the heavens found to be very nearly, when their colours appear strong and perfect. For once, by such means as I then had, I measured the greatest semidiameter of the interior iris, about 42 degrees, and the breadth of the red, yellow, and green in that iris 63 or 64 minutes, besides the outmost faint red observed by the brightness of the clouds, for which we may allow 3 or 4 minutes more. The breadth of the blue was about 40 minutes more, besides the violet, which was so much obscured by the brightness of the clouds that I could not mea-

sure its breadth." And again, "The least distance between this iris and the exterior iris was about 8 degrees and 30 minutes." We see that the fundamental measures are for the extreme red of the primary bow, which has its utmost radius $42^{\circ} 4'$, and the *least* distance between this and the corresponding part of the secondary bow $= 8^{\circ} 30'$. Now, Sir Isaac Newton has taken $\frac{4}{3}$ for the refractive index of this extreme visible red, which brings, as he says, the computations to agree nearly with the observations; but from Fraunhofer's correct tables of refractive indices we find that it coincides very nearly with the letter *D*, or the middle of the orange. After all,—with this calculation for orange and measurement for extreme red, there is a discrepancy of no less than $14'$, for with refractive index $= \frac{4}{3}$, we have radius (if the sun were a point) $= 42^{\circ} 2'$, and adding sun's semidiameter $= 16'$ we have $42^{\circ} 18'$, differing $14'$ from the extreme measurement; the measured radius being less than the calculated one.

Before we can proceed with the correct calculation we must fix on some refractive index as corresponding to the extreme visible red; and the letter *C* being about the middle of the red, if we take about the letter *B* this will not be pushing the case too far, and will probably err in the favour of the old theory rather than otherwise.

With this refractive index $= 1.3309$ we find for the radius of the primary bow, if the sun were a point, $42^{\circ} 23'$, and adding sun's semidiameter, we have radius of extreme visible bow $= 42^{\circ} 39'$, and the difference between this and the observed quantity $42^{\circ} 4'$ is $35'$, a quantity far beyond the limits of error of observation.

The distance between the bows, however, shows the discrepancy in the strongest point of view, on account of its bearing so large a ratio to the whole distance. The above refractive index gives for the radius of the extreme red of the secondary bow $50^{\circ} 20'$, and the difference between this and $42^{\circ} 23'$ is $7^{\circ} 57'$; subtracting from this the sun's semidiameter for each bow, or $32'$ for both, we have $7^{\circ} 25'$, whilst the observed distance is $8^{\circ} 30'$, leaving a discrepancy of no less than $1^{\circ} 5'$ in this small angle, which cannot possibly be attributed to faults in the measures.

To test Mr. Airy's conclusions that the light at the position of the primary bow, according to the old theory, should be about one half of that at the brightest part if the sun were a point, and therefore distinctly bright in the real rainbow, we will allow a greater latitude in favour of the theory, and take the letter *C* refractive index $= 1.33171$ as belonging to the extreme visible red; this gives the radius of the primary bow $42^{\circ} 32'$, whilst the observed extreme bow has only a radius

42° 4', leaving a discrepancy of 26', or there is no visible light at that position indicated, instead of being of great brightness.

Again, for the corresponding radius of the secondary bow we find 50° 17', and the difference between this and 42° 32' is 7° 45', so that according to Mr. Airy the light should be strongly visible to points distant 7° 45' from each other; whilst the utmost limit to which the light can be traced is still distant 8° 30', and this difference leaves a strong presumption against the theory, and induces a corresponding argument in favour of the corpuscular theory, with which this fact is in accordance.

III. *Meteorological Observations taken at St. George's, Bermuda, in the December half-year of 1837; introduced by Corrections of Observations for the June half-year. By Lieut.-Col. EMMETT, R.E.**

MEAN height of barometer (Lond. & Edinb. Phil. Mag. Nov. 1837.) should be for July 30·069, and not 30·161, which is the height not corrected. The mean heights for August and September should each be reduced ·020, ·054 having accidentally been added for capillary action instead of ·034.

In the horary differences forwarded to Dr. Dalton it will be observed the numbers of observations at the different hours vary; they were also not always made on the same days: but in revising my journal I selected those days only for 9 A.M. and 4 P.M., on which the observations were made for both hours; and upon that revision the tables in your January number were prepared. Such has been the course in the half-yearly observations now inclosed, completing the year 1837. It is worth notice that the general fall of the barometer from 1 P.M. to 4 P.M. usually and considerably exceeds that from 9 A.M. to 1 P.M. Page 46, lines 4 and 5, should be subtracted from the "former," not the latter.

From a frequent comparison of the dew-point *directly taken* with the wet-bulb thermometer I feel much confidence in it, where suspended in a large room free from currents and fully open to the air; but where exposed to currents, reflections from the ground, &c. it cannot be fully relied upon, both it and the register thermometer being affected thereby, and this it is often difficult to avoid. As I before stated, the difference between the wet and dry \times by 2 and taken from the height of the dry, gives the dew-point nearly, but rather too low, as the multiple should be -2 ; errors are therefore nearly doubled.

Meteorological Observations taken at St. George's, Bermuda, in the December half-year of 1837.

Month.	Barometer.				Thermometer.				Tempera- ture of Sea.	Hygrometer.		Rain.				Gales.	Winds, &c.	
	Mean.	Max.	Min.	Diff.	Mean.	Max.	Min.	Diff.		Dew Point.	Vapour.	Day.	Night.	Total.	Prevailing.			
July.	30-031	30-398	29-908	·490	76	81-5	69-5	12	79-5 82	70-3	·728	2-09	2-41	5-40	2	1st SW. 2nd NW.	Th. & Light- ning	
August.	30-071	30-366	29-936	·430	78-2	83-5	72	11-5	81 84	69-2	·702	·01	·73	·74	1	1st NW. 2nd SW. 3rd SE.	Th. & Lig- ht. only	
Sept.	30-117	30-360	29-986	·374	76-61	83-5	71-5	12	82 79-5	66-48	·634	·15	1-00	1-15	1	1st NE. 2nd SW. 3rd SE.	Light & Th. Light. only	
Oct.	29-977	30-330	29-594	·706	71-95	78-5	64-5	14	78 75-5	66-30	·627	3-80	6-05	9-85	3	1st NE. 2nd SE. 3rd SW.	Li. & Th. 3. Li. only 6.	
Nov.	30-107	30-430	29-736	·694	65-99	73-5	59	14-5	69 67-5	58-25	·494	1-92	2-37	4-29	3	1st SW. 2nd NE.	Li. & Th. 2 Lightning 3.	
Dec.	30-037	30-406	29-710	·696	62-25	69-5	56	13-5	67-5 64	55-51	·454	2-55	3-85	6-40	9	1st NW. 2nd NE.	Th. & Light not observed	
June half Year.	30-113	30-381	29-810	·571	71-83	78-33	66-41	11-92	75-8	64-34	·607	11-42	16-41	27-83	19			
Year 1837	30-053	30-378	29-727	·651	67-18	74-66	60-5	14-11	71-35	61-02	·542	22-54	32-61	55-15	63			

Horary Changes of Barometer.

Months.	No. of Observations.	9 A.M.	4 P.M.	Diff.	Times higher at 4 than at 9.	No. of Observations.	9 P.M.
July.	29	30.121	30.113	.008	10	28	30.126
August.	29	30.172	30.156	.016	6	25	.171
Sept.	26	30.185	30.153	.032	5	19	.171
Oct.	28	30.075	30.046	.029	4	18	.043
Nov.	29	30.192	30.157	.035	6	10	.048
Dec.	27	30.075	30.057	.018	8	16	.093
	168	30.137	30.113	.024	39	116	30.105
June Half year.	156	.057	.025	.029	27	108	.043
Whole year.	324	30.097	30.069	.027	66	224	30.074

Mem.—The observations at 9 a.m. and 4 p.m. were all taken on the same days; the number of times when higher at 4 than at 9 a.m. are included in those of column 2.

In August the barometer was lower at 5 p.m. .013 than at 4 p.m. In September 4 p.m. gave the minimum, which continued so until near December, when 3 was at times lower than 4 p.m.

Nine a.m. was almost always higher than 8 or 10.

Winds.

Months.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
July.	4	2	1	6	6	45	3	26
August.	5	8	4	16	6	20	6	28
Sept.	4	24	6	21	6	23	1	5
Oct.	13	26	8	16	8	10	3	9
Nov.	8	23	6	10	6	27	3	7
Dec.	4	24	1	7	2	10	5	40
	38	107	26	76	34	135	21	115
June half year.	53	70	13	50	44	126	37	145
Whole year.	91	177	39	126	78	261	58	260

Observations on the year 1837.

	Not corrected.	Corrected and reduced to 32°.
Barometer highest on 15th Feb. }	30·482	30·457
Wind SE., light. Therm. 56° }		
Lowest 19th Jan. SE. heavy and }	29·394	29·339
suddenly changing to NW. Th. 61 }	<hr/>	<hr/>
	1·088	1·118
Mean height corrected	30·053	
Deduct mean of vapour	·542	
	<hr/>	
	29·511	
Mean heat ...	67·18	
Greatest	83·5	
Least	47·	

Observations on the last six months.

Solar heat (greatest on blackened bulb therm.)

July	Aug.	Sep.	Oct.	Nov.	Dec.
107°	115°	103°	98°	89°	88°

On the 14th of November an aurora borealis was seen between 6 and 7 p.m. centring on the north. Streamers extending towards the Pleiades, and converging towards Cassiopeia, never rising so high. Stars, except of first magnitude, not seen through it. Colour, except streamers, a deep red. At its close, about 7 p.m., a haze covered the whole space, and there was lightning in the SW. Barom. 30·024. Therm. 72°. Wind SW. fresh. During the appearance the gusts of wind were frequent and strong.

The barometer had been gradually falling from the 10th, when it was 30·400; at 4 p.m. of the 14th it was down to 29·998.

Bermuda, March 31st, 1838.

IV. *Experimental Researches on the Nature and Properties of Albumen, &c.* By GOLDING BIRD, M.D., F.L.S., G.S., &c.
Lecturer on Experimental Philosophy at Guy's Hospital.*

IN my former papers on this subject†, I have given an account of some of the properties of free and combined albumen, chiefly in relation to carbonic acid and electric currents; and I hazarded a remark that these investigations would probably serve to point out the presence of albumen in certain

* Communicated by the Author.

† [See Lond. and Edinb. Phil. Mag. vol. ix. p. 109.]

animal fluids in which its presence was unsuspected, and thus bring us acquainted with some new combinations of this important product of organization: this remark has certainly received some support from the facts that have fallen under my notice during the last three years, and which I propose to detail in this paper. In the last number of the Guy's Hospital reports, I have related such of my experiments as appeared to be connected with the physiological bearings of this subject, I shall therefore confine myself in this communication to the strictly chemical investigations, in the hope that they will assist in removing the obscurity hanging over that part of animal chemistry connected with the *peculiar* and ill-defined organic principles existing in mucus, saliva, &c., and stated by most chemical writers to differ from albumen in their chemical characters.

1. Mr. Brande, in a paper published in the Philosophical Transactions for 1809, first demonstrated the existence of albumen in saliva and mucus, and it is a matter of regret that this philosopher has not followed up these views, as much light must have been thrown on this subject by researches guided by his talent. The state of combination in which the albumen existed in mucous fluids Mr. Brande failed to discover, although he remarked that in some features it resembled very closely an alkaline albuminate. Before giving an account of the results obtained by a repetition of his interesting experiments, it will be proper to describe the behaviour of those secretions, in which the presence of albumen was suspected, towards re-agents, as it will serve to bring us acquainted with a combination of albumen, differing from any described in my previous papers. For this purpose, saliva, or any other mucous fluid which *does not coagulate* by heat, may be taken as an example, and perhaps that viscid glairy fluid secreted during the first few days of acute bronchitis is the best for our purpose, as from its abundant secretion, and the frequency of the disease, it can be obtained in sufficient quantity for satisfactory chemical investigation. This form of mucus appears at first to be rather opaque, from the presence of innumerable air-bubbles, but by repose in a cylindrical vessel these rise to the surface, and a nearly limpid fluid is obtained: this does not coagulate by heat, and presents the following appearances with re-agents:—

A. On the addition of sulphuric acid a reddish-brown solution is formed; which by dilution with water loses its colour, and becomes quite transparent.

B. Nitric acid appears *at first to coagulate it*, rendering it yellow in patches, and by the assistance of heat forms a pale yellow solution, becoming brown on the addition of an alkali.

C. Hydrochloric acid removes the slight turbidity it previously possessed, *and causes it to assume a lilac tint.*

D. Ammonia, by the assistance of heat, partly dissolves it; forming a gelatinous solution, becoming turbid when diluted with water.

E. Acetic acid produces a partial coagulation; causing the mucus to assume the appearance of a corrugated membrane, floating in the acid.

F. Infusion of galls produces a copious precipitate.

G. A quantity being evaporated to dryness, left a gum-like residue; which, when carefully incinerated in a platinum crucible, yielded a perfectly white ash, destitute of all traces of iron: it turned turmeric paper brown, and partly dissolved in acids with effervescence, demonstrating the presence of an alkaline carbonate.

Upon a review of these experiments, we find none of the phenomena hostile to the opinion of the presence of albumen: indeed, some of them (B. C. F.) appear to indicate the probability of its existence. The action of acetic acid (E.) is quite peculiar to that combination existing in mucus; to which I shall again have occasion to refer.

2. When nearly limpid mucus possessing the above properties is kept for a few days exposed to the air, it becomes turbid, and gradually lets fall a white cream-like deposit. Some of this was collected and examined: it presented, under the microscope, the appearance of numerous round particles, which were readily recognised as coagulated or insoluble albumen; for they dissolved in hydrochloric acid, yielding a fine lilac-coloured solution; in nitric acid, with the aid of heat, they formed a yellow fluid, becoming brown on the addition of potass; and with acetic acid they yielded a colourless solution, from which ferrocyanide of potassium threw down a pale yellow precipitate in the cold.

3. Having thus proved this deposit to consist of albumen, a most interesting question arises as to its source; for, as has been already shown, (2) none of this principle could be absolutely *proved* to exist in the mucus before exposure to air, however much its presence might have been *suspected*. My first suspicions were, that the carbonic acid of the atmosphere had been the active agent, by combining with the substance which previously held the albumen in solution: this appeared to be probable, as some experiments, which it is unnecessary to detail, seemed to countenance the idea of the existence of albuminate of soda in mucus. I accordingly placed a glass filled with the same limpid mucus under a jar of hydrogen

gas, and in a few days the same creamy deposit appeared as when exposed to the free air; hence proving satisfactorily, that the absorption of carbonic acid is not necessary for producing this curious change.

4. When simple limpid mucus is boiled in a test tube, no coagulation, as already stated, takes place; but on 'prolonging the ebullition, a milkiness appears, and after a few minutes an insoluble opake deposit ensues. This change is best observed by heating two or three ounces of the mucus over a lamp in a glass basin: the deposit is then more distinct, and by repose it becomes considerable. On examination, it is found to consist of albumen in amorphous particles, in which alone it differs from that precipitated from mucus by exposure to the air, or to an atmosphere of hydrogen. This change has been mentioned by Dr. Pearson *, although he did not examine the nature of the deposit.

5. Another variety of mucous secretion, of frequent occurrence, is that termed purulent or puriform mucus, secreted copiously during chronic bronchitis. This is generally very opake, often containing greenish masses, exceedingly tenacious; so that on attempting to pour it from one vessel to another, instead of falling in drops, it forms one continuous rope, sometimes two or three feet in length, which is absolutely sectile. It usually contains innumerable air bubbles, which are evolved with difficulty: it can be scarcely said to be miscible with water, on account of its excessive tenacity. It bears considerable resemblance to simple mucus concentrated by evaporation, after having deposited part of its *combined* albumen by exposure to air. The behaviour of purulent mucus with various reagents, therefore, exactly resembles what we should, *à priori*, expect from operating on simple mucus, holding numerous minute particles of insoluble albumen in intimate diffusion. In general I have observed all the varieties of mucus to exert a faint but distinct alkaline reaction on syrup of violets, and on paper tinted with infusion of rose petals; and after a few days' exposure to the air this effect becomes still more obvious. Dr. Babington has shown in a late paper† on this subject, that the bronchial mucus is constantly alkaline; and my friend Mr. Richard Phillips lately informed me that he had been long aware of this circumstance, having demonstrated it in the saliva by means of cudbear paper.

6. The globules or particles of insoluble albumen present in puriform mucus are sufficiently obvious under a moderate

* Philosophical Transactions, 1809, p. 322.

† Guy's Hospital Reports, vol. ii. p. 539.

magnifying power. When acted on by reagents the following were the results.

A. Sulphuric acid formed a pale reddish solution with purulent mucus; remaining nearly transparent, after dilution with water.

B. Nitric acid dissolved it with great difficulty, and not until after the application of heat: a pale yellow solution was then obtained, becoming orange red on the addition of a solution of potass.

C. Ammonia by the assistance of heat yielded a turbid solution: when this was poured into cold water, it formed, after a few seconds' repose, a thin layer on the surface; which, when viewed from above downwards, appeared quite diaphanous, but when placed horizontally between the eye and the light, appeared like a layer of semi-opaque jelly: this after some time subsided in a manner closely resembling the subsidence of silicic acid from hydro-fluosilicic acid, after the addition of a potass-salt.

D. Hydrochloric acid partly dissolves puriform mucus, forming a lilac-coloured troubled fluid.

E. Acetic acid did not dissolve it, even after the application of heat: it appeared to contract the mucus into a corrugated membranoid mass, which floated on the surface of the fluid.

F. By careful incineration in a platinum crucible, a nearly white ash was obtained, which exerted an alkaline action on turmeric paper, and partly dissolved with effervescence in dilute acids. It is unnecessary to make any remark upon these reactions, as they resemble those produced by simple mucus, modified only by the presence of particles of free albumen.

7. Some ounces of rather opaque mucus of bronchitis were placed in a flask furnished with a tube bent twice at right angles, and immersed in lime water. A lamp heat was then applied to the flask; and in a few minutes, long before actual ebullition, bubbles of gas were copiously evolved from the mucus; and on passing through the lime water, they rendered it quite milky from a copious deposit of carbonate of lime; hence proving most satisfactorily the presence of carbonic acid in mucus, either free, or in so loose a state of combination as to be evolved by a very gentle heat.

B. Two fluid ounces of similar mucus were mixed with a small quantity of a solution of potass, in a flask furnished as before with a bent tube, the end of which was immersed in a small quantity of pure dilute hydrochloric acid. On applying heat to the flask, the upper part and tube became soon filled with white fumes; and after five minutes' boiling the lamp was removed: the dilute acid being carefully evaporated to

dryness in a glass capsule, yielded numerous delicate feathers of hydrochlorate of ammonia. To obviate any source of fallacy in this experiment, some of the same specimen of diluted hydrochloric acid was evaporated to dryness; but scarcely the minutest traces of residue were visible even with the aid of a lens. By simple distillation without admixture, mucus (as stated by Dr. Pearson) does not yield the smallest trace of ammonia; hence we may very safely conclude, that although mucus does not contain free ammonia, yet it contains a salt of that base, most probably the chloride, (muriate of ammonia) a salt which the ingenious researches of Raspail* have proved to be almost universally present in animal fluids.

8. Some fresh saliva, obtained without the use of any chemical stimuli, was mixed with an equal bulk of water, and after violent agitation filtered; this fluid was quite limpid, but by exposure to the atmosphere in an imperfectly closed jar during forty-eight hours, an opacity occurred precisely as in the case of mucus (2), the substance deposited not being soluble in nitric acid: the same thing occurred by exposure to an atmosphere of hydrogen gas. Some of this limpid dilute saliva was exposed to heat in a glass tube; no coagulation occurred: but by protracted ebullition in a glass basin numerous flocculi were deposited; these flocculi could not be distinguished from coagulated albumen by their behaviour towards reagents.

9. Some fresh human saliva was filtered, and exposed in a glass cup by means of copper wires to a current of electricity from a battery of six pairs of plates two inches square, excited by weak brine in the manner described in my last communication on this subject (18): coagulation ensued in a few seconds around the *positive* electrode; the coagulated mass adhered to the wire, and acquired a green tint from the oxidation of the copper electrode. Another portion of saliva was submitted to the electrolytic action of an electric current, in two cups connected by moistened cotton: coagulation very soon took place at the same electrode as in the last described experiment.

10. Mucus diluted with water and filtered, presented the same appearances when acted on by a voltaic current as saliva, coagulation constantly taking place, and at the *positive* electrode.

11. When the coagulated substance separated by electric action (9 and 10) from mucus or saliva was examined chemically, in no single feature was it found to differ from ordinary coagulated albumen: in its solubility in acetic acid, alkalies, &c., not the slightest discrepancy could be detected.

* *Nouveau Système de Chimie Organique*, pp. 195, 346, &c.

12. From all the foregoing experiments we learn, that fluids, as mucus and saliva, which do not give indications of the presence of albumen in a satisfactory manner by the application of ordinary reagents (1 and 6), by exposure to the air (2 and 8) or to an atmosphere of gaseous hydrogen (3, 8) let fall a deposit in the form of minute particles, which resembles coagulated albumen so closely that we are hardly justified in considering them as distinct. The same fluids when traversed by an electric current of low intensity (9, 10) give up, at the surface of the positive electrode, a white substance, which no chemical reagent to which it has been exposed can distinguish from artificially coagulated albumen. The results thus obtained by the action of electricity are corroborative of those obtained by Mr. Brande twenty-eight years previously, the only discrepancy that exists depending upon the surface at which the coagulation took place: to this I should beg to propose a similar explanation to that which I have suggested in my former paper with regard to albumen (23).

13. In consequence of the separation of *coagulated albumen*? from saliva by an electric current, the existence of the *peculiar* animal matter of saliva (ptyalin of Berzelius) might appear questionable, and it appeared probable that it might be some albuminous combination instead of a distinct proximate principle. To determine this, some *ptyalin* was prepared by evaporating some saliva to dryness, digesting the residue in hot alcohol, and then in cold alcohol acidulated with acetic acid. The insoluble residue, which notwithstanding its washing with alcohol was acid, partly dissolved in water, leaving an insoluble substance very closely resembling, if not identical with, coagulated albumen. The watery solution contains, according to Berzelius, (*Traité*, v. 6.) tolerably pure *ptyalin*. This solution was placed in two glass cups connected by moistened cotton, and exposed by means of platina wires to the action of an electric current from 36 pairs of plates two inches square excited by weak brine: coagulation ensued at both electrodes, most freely at the negative side. This coagulated substance could not be distinguished from albumen, hence it appeared probable that the *ptyalin* of Berzelius consisted of some hitherto unknown combination of that principle.

As it is obvious that no previously known combination of albumen would present all the phænomena of the (so called) *peculiar proximate principles* of mucus or saliva with reagents, although its solutions in alkalies and in carbonic acid would present some, it became a most interesting inquiry to seek after and develop the nature of this unknown combination.

This was a task of no slight difficulty, on account of our very limited acquaintance with the nature and properties of the aluminous combinations found in the different secretions.

[To be continued.]

V. *On the Composition of certain Mineral Substances of Organic Origin.* By JAMES F.W. JOHNSTON, M.A., F.R.SS. L. & E., F.G.S., Professor of Chemistry and Mineralogy, Durham.*

V. *Elastic Bitumen of Derbyshire.*

THE elastic bitumens of Derbyshire and Montrelais have been analysed by Henry jun. He found them to consist of

	From Odin Mine.	From Montrelais.
Carbon	52·250	58·260
Hydrogen...	7·496	4·890
Nitrogen ...	0·154	0·104
Oxygen.....	40·100	36·746

100·

100·

Journal de Chimie Medicale, i. p. 18.

This analysis is open to two remarks; first, on the excessive, and in the present state of manipulation the almost impossible, refinement of estimating one tenth of a per centage of nitrogen; and second, on the large amount of oxygen which it indicates. This quantity is so much beyond what we should expect from the appearance of the substance itself, from its chemical relations, and from the circumstances under which it is met with, as at once to awaken doubts of the accuracy of the analysis.

I have analysed three varieties of the elastic bitumen of Derbyshire, and have obtained a widely different result.

1. The first was soft, elastic, adhering to the fingers, yielding to slight pressure, of a brown colour and a strong peculiar odour. At 212° Fahr. it decreased in weight, giving off a volatile matter possessing the unpleasant odour of the mass.

10·052 grs. burned with oxide of copper gave 31·07 grs. of carbonic acid, and 12·018 grs. of water. These are equal to

Carbon.....	85·474
Hydrogen	13·283

98·757

* Communicated by the Author.

The loss may either be due to the presence of oxygen, or to the extraction of a trace of the more volatile part when heated, before burning, for the purpose of pumping out the water.

2. The second, of which I had a larger supply, had a close resemblance to moderately soft India rubber. Its colour was of a darker brown. Boiled in water its colour became paler, but it again darkened on drying at 212° . During the boiling a more volatile portion collected on the surface of the water and the sides of the flask, which on cooling presented the appearance of a very soft white, or slightly brownish solid. At 212° in the air it also diminished in weight. Boiling alcohol and æther extracted from it a similar volatile substance, but very sparingly, and of a browner colour. I did not recognise in this substance the bitter taste remarked by M. Henry. I have in my possession, however, a substance of a similar kind from South America, which I shall describe in a future Number, possessed of an intensely bitter taste, a trace of which may, perhaps, be occasionally present in the elastic bitumen, and give a bitterness to the matter extracted from it by æther.

Of this second or harder variety, 13·66 grs., cut into small pieces and boiled once in æther and three times in alcohol, lost 2·46, or 18·008 per cent. It still retained its elasticity after this treatment.

Of the portion thus boiled, 11·195 grs. gave on burning 34·165 grs. of carbonic acid, and 12·67 grs. of water. Of a second portion, first boiled for a long time in water, and afterwards in successive portions of alcohol, as long as anything seemed to be taken up, 8·74 grs. gave 26·447 of carbonic acid, and 9·86 of water. These are equivalent to

	1st.	2nd.
Carbon	84·385	83·671
Hydrogen	12·576	12·535
	<hr/>	<hr/>
	96·961	96·206

And indicate the presence of from three to four per cent. of oxygen in the portion of the bitumen which remains after the action of alcohol and æther.

3. The soft elastic bitumen is said by long keeping to become hard and brittle. It is certain that portions of this brittle kind occasionally occur imbedded in the softer mass.

Of a specimen of this brittle variety having a vitreous lustre and conchoidal fracture,

- a. 6·263 grs. gave 19·47 carbonic acid, and 6·957 of water.
 b. 5·93 grs. gave 18·48 ————— — 6·63 —————

These are equivalent to

	1st.	2nd.
Carbon	= 85.958	86.177
Hydrogen	= 12.342	12.423
	<hr/>	<hr/>
	98.300	98.600

It would appear that this variety also contains a small quantity of oxygen.

These analyses show us,

1. That the elastic bitumens are very nearly akin to the Hatchetine and Ozocerite, and are probably an equiatomic carbohydrogen (CH) slightly altered.

2. The first compared with the succeeding analyses shows that in the soluble and more volatile of the two portions of which these bitumens consist, the carbon and hydrogen are more nearly in atomic proportion than in the elastic insoluble portion, and render it probable that the soluble part is a variety of Hatchetine or Ozocerite, of which originally the entire mass consisted; and therefore,

3. That the change which the originally pure carbohydrogen has undergone, has either been the result of a decomposition analogous to that which many of this group of carbohydrogens are known to undergo, or of an oxidation to a small extent, perhaps of both. If oxidized it may either be so by the direct addition of oxygen to the unchanged compound, or by the replacement of a portion of its hydrogen, in which case the atomic ratio of the fundamental elements must be altered. Were we certain that the second variety analysed was wholly free from mixture, the ratio of its elements might be calculated and the true nature of the change determined; while doubt remains in regard to its purity, however, the result of such a calculation would be deserving of little confidence.

In regard to the origin of this substance, I am inclined to attribute its presence in the mineral veins and fissures which traverse the mountain limestone in Derbyshire, to sublimation from beneath. The immense stratiform deposits of trap which traverse that district, indicate a sufficient cause for such sublimations. The contact of a fused lava with the organic matters which abound in the strata of the carboniferous æra, could not fail to cause the evolution of volatile substances, which would condense when they reached a colder region. Bituminous substances are found also in the carboniferous limestone in Fifeshire, where trap rocks are known to penetrate or disturb the strata; and it is not unlikely that in most cases their appearance near the surface is due to a high temperature,

derived from some similar source, acting on substances either themselves organized, or like coal of organic origin.

Durham, May 1838.

VI. *On the Separation of the Oxalic from other Organic Acids.*

By JAMES F. W. JOHNSTON, M.A., F.R.SS., L. & E.
F.G.S., *Professor of Chemistry and Mineralogy, Durham.**

AT the Liverpool meeting of the British Association in September last, I exhibited and stated the composition of a beautiful salt I had formed some months before, consisting of an atom of nitrate with an atom of oxalate of lead and two atoms of water. In the Number of the Philosophical Magazine for May is given an extract of a paper by M. De-jardin, in which this salt is very correctly described, and certainly without the knowledge of my having previously made it known†. As the study of this interesting compound, however, has suggested an easy method of separating the oxalic from other organic acids, I shall briefly describe the unpublished observations I have made upon it.

I prepare the salt by adding nitric acid in considerable quantity to a weak solution of oxalic acid, or of acetate of lead, and pouring in slowly a solution of subacetate of lead, or of dilute oxalic acid. Shining plates of the compound speedily begin to fall. If the quantity of oxalic acid be minute, or if it be largely diluted, the crystals fall only after some time, and in the form of six-sided tables, of which two of the sides are longer than the others, possessing a silvery whiteness and pearly lustre, and striated longitudinally so as to exhibit the most beautiful prismatic colours when light is reflected from them. I have also obtained it in acicular prisms nearly an inch in length, which according to the measurement of Professor Miller of Cambridge, are oblique rhombic prisms.

Decomposed by sulphuretted hydrogen it gives a colourless solution, which, when evaporated, emits fumes of nitric and yields crystals of oxalic acid.

Heated to 212° this salt does not diminish in weight; at a temperature of about 500° Fahr. it loses 2 atoms (5.425 per cent.) of water, and before it reaches 570° Fahr. it has given off copious red fumes, lost upwards of 19 per cent., and is

* Communicated by the Author.

† I may take this opportunity of mentioning, that under the name of Iodal, M. L'Amy has lately described a compound of which I published an account in the Edinburgh Journal of Science (II. p. 415.) some years ago. It is obtained by the action of nitric acid on iodine in alcohol, and has not yet been analysed.

wholly converted into carbonate of lead. At a higher temperature the carbonic acid is driven off.

Water decomposes it, extracting when boiled over the salt the greater part of the nitrate, and leaving nearly pure oxalate; a small portion of the double salt being dissolved at the same time, which precipitates again on cooling. Thus 24·26 grs. boiled in water left 12·70 grs. of insoluble residue, or 52·35 per cent.; the quantity of oxalate in the salt being only 50·016 per cent. When newly precipitated, or before it has been dried at 212°, it is much more readily decomposed, so that it cannot be washed on the filter even with dilute nitric acid without decomposition.

10·59 grs. heated to nearly 500 Fahr. lost before any trace of red fumes appeared 0·56 = 5·28 per cent. of water. Three successive portions on heating to redness left respectively 67·51, 67·55, and a purer variety 67·28 per cent. of oxide of lead.

These results agree with the formula $\text{Pb}^{\text{N}}\text{N} + \text{Pb}^{\text{C}}\text{C} + 2 \text{HO}$.

	Exper.	Calcul.
Or, Oxide of lead	67·28	67·312
Acids	37·54	37·263
Water.....	5·28	5·425
	<hr/>	<hr/>
	100·	100·

I met with this salt in the course of an examination of the action of nitric acid on certain organic substances. Thus if oil of turpentine or of lemons, the balsams, the sugars, colophony, elemi, gamboge and other resins, Burgundy pitch, or indigo be boiled in nitric acid, either dilute or concentrated, and to the acid solution, from which the yellow resin formed during the operation has been precipitated by water, sub-nitrate of lead be added, the new salt falls in great abundance, indicating the production of oxalic acid. Not suspecting the crystals I obtained in this way to be a double salt, I was at much pains in making out their elementary composition by burning with oxide of copper, and I only prepared it directly from a mixture of oxalic and nitric acids after I had completely analysed it. During the action of nitric acid, however, on some, if not upon all the organic substances above mentioned, other acids are formed; and this is more especially known to be the case in regard to indigo. These acids are held in solution along with the oxalic, and continue to be so held after the addition of subacetate of lead to the acid liquid ceases to throw down any more of the double salt. It becomes interesting then to examine how far the whole of the oxalic acid

could be thrown down by this means while the solution still remained acid.

1. If into solutions of acetic, tartaric, citric, carbazotic, indigotic, benzoic, succinic, gallic, meconic, pyromeconic, mucic, or camphoric acids, nitric acid be poured and afterwards subacetate of lead, or if nitric acid be added largely to solutions of acetate or nitrate of lead and solutions of these acids be dropped in, the precipitate at first formed speedily redissolves, and no further precipitate, crystalline or otherwise, appears on standing for any length of time. I have not tried any other organic acids, but the same is probably true of many of them also.

2. But if into a solution thus prepared, and containing already one or more of these acids, a few drops of a solution of oxalic acid be introduced, crystals of the double salt begin to appear.

3. This is beautifully illustrated, and at the same time the conversion of tartaric into oxalic acid, by dissolving the former or the bitartrate of potash in dilute nitric acid; the solution gives no precipitate with subacetate of lead, but boil it a little, and a precipitate in shining crystals appears on adding the salt of lead. This forms a very instructive class experiment. Care must be taken to have the solution sufficiently acid, or more or less of pure oxalate of lead will accompany the double salt.

4. 15.92 grs. of tartaric and 6.08 grs. of oxalic acid with one atom of water ($\text{C} + \text{H}$) were dissolved in a small quantity of water, and poured into an acid concentrated solution of nitrate of lead in large excess. The double salt collected and dried at 212° Fahr. weighed 45.03 grs., equivalent to 6.14 grs. of ($\text{C} + \text{H}$). This indicates 0.06 of oxalic acid in excess, an error which can hardly be avoided, from the impossibility of sufficiently washing the precipitate and filter without risking decomposition.

5. But the oxalic acid may also be separated from all these other acids, and estimated with tolerable precision, without the formation of the double salt. Thus 10 grs. of oxalic acid in

crystals ($\text{C} + 3 \text{H}$), equivalent to 5.73 of anhydrous acid, were mixed with 20 of tartaric acid, 10 of citric, 2 of benzoic, 4 of succinic, and an unmeasured quantity of acetic acid dissolved in two ounces of distilled water, and acid nitrate of lead added to the solution. By this method of proceeding a sufficient quantity of nitric acid was present to prevent the tartrate,

benzoate, &c. from falling, but not enough to cause the formation of the double salt. A crystalline precipitate, indeed, fell, but it was only oxalate, with a few flakes of the double salt. After drying at 212 it weighed 23.16 grs. and lost when heated to redness 26.85 per cent. Pure dry oxalate contains 24.68 per cent. of acid; and 23.16 grs. of oxalate are equivalent to 5.71 of anhydrous acid, very nearly the quantity employed. Some time must be allowed for the perfect deposition of the whole of the oxalate or of its double salt.

From these experiments it appears that by simply acidifying strongly with nitric acid, the oxalic may be separated almost completely from solutions containing any of the other organic acids above enumerated, and its quantity determined with considerable accuracy.

The utility of this process in separating the oxalic acid, formed so largely in the preparation of the indigotic and carbazotic acids and other highly oxidized compounds, need not be pointed out, nor the means it affords us of estimating *quantitatively* the nature of the changes produced on organized bodies by the action of oxidizing agents.

Durham, May 29, 1838.

VII. *Of the Reaction of the Essential Oils with Sulphurous Acid, as evolved in union with Æther in the Process of Ætherification, or otherwise. By R. HARE, M.D., Professor of Chemistry in the University of Pennsylvania*.*

HAVING mixed and subjected to distillation two ounces of oil of turpentine, four ounces of alcohol and eight ounces of sulphuric acid, a yellow liquid came over, having all the appearance of that which is obtained in the process for making oil of wine, described in a preceding article. On removing, by means of ammonia, the sulphurous acid existing in the liquid, and driving off the æther by heat, a liquid remained, which differed from oil of turpentine in taste and smell, although a resemblance might still be traced. This liquid was without any sensible action on potassium, which continued bright in it for many weeks. It proved, on examination, to contain a small quantity of sulphuric acid. I ascertained, afterwards, that in order to produce these results, it was sufficient to pour oil of turpentine on the mass which remains after the termination of the ordinary operation for obtaining æther, and apply heat. Subsequently it was observed that when the sulphurous æther was removed by heat

* From the Transactions of the American Philosophical Society, N.S. vol. v.

or evaporation, without the use of the ammonia, the proportion of sulphuric acid in the remaining oil was much greater.

By subjecting to the same process several essential oils, I succeeded in obtaining as many liquids to which the above remarks were equally applicable. With some of the oils, however, similar results were, by this method, either totally or partially unattainable, in consequence of their reaction with the sulphuric acid being so energetic as to cause their decomposition before any distillation could take place. No product can be obtained by distillation with sulphuric acid and alcohol from the oil of cinnamon obtained from cassia. From the oils of sassafras and cloves, but little can be procured.

However, in one instance, by previously mixing the oil of sassafras with the alcohol, in the manner described in the account given of the first experiment with the oil of turpentine, I succeeded in obtaining in addition to a small quantity of the heavy liquid containing sulphuric acid, a minute quantity of a lighter one, devoid of that acid, which burned without smoke, was insoluble in water, and very fluid. I am disposed to consider the liquid thus procured as a hydrate of sassafras oil, or sassafraine, as I would call it, being analogous to hydric æther.

The oil of sassafras, whether isolated or in combination, possesses a remarkable property, which, I believe, has not attracted sufficient observation: I mean that of producing an intense crimson colour, when added, even in a very minute quantity, to concentrated sulphuric acid.

One drop of oil of sassafras imparted a striking colour to forty-eight ounce measures of sulphuric acid, and appeared perceptible when it formed less than a five millionth part. This property was completely retained by the lighter liquid above described as procured from oil of sassafras.

I subsequently observed, that when sulphurous acid, whether in the form of sulphurous æther, in that of a gas, or when in union with water, was brought into contact with any of the essential oils (including kreosote), which were subjected to the experiment, they acquired a yellow colour, and a strong smell of this acid.

In the case of the yellow compound thus obtained from any of the essential oils which I have tried, if the sulphurous acid be removed by heat, the oil, by analysis, will be found to yield sulphuric acid. That some acid of sulphur remains in union must be evident, since washing with ammonia will not entirely remove the power of yielding sulphuric acid; and the total absence of the sulphurous smell demonstrates that

the sulphurous acid either enters into an intimate combination with the oil, or acquires oxygen sufficient to convert it into sulphuric or hyposulphuric acid.

Those essential oils which contain oxygen, are most affected by the action of sulphurous acid.

Both the oils of cloves and cinnamon, after admixture with sulphurous æther and subsequent distillation, gave, on analysis, precipitates of sulphate of barytes. In the case of cloves, the precipitate amounted to one-seventh of the whole weight.

By distilling camphor with alcohol and sulphuric acid, I obtained a yellow liquid, which, by washing with ammonia and evaporation, in order to get rid of the sulphurous æther, yielded an oil. The oil, by standing, separated into two portions, one solid, the other liquid. The solid portion resembled camphor somewhat in smell, but differed from it by melting at a much lower temperature, becoming completely fluid at 175°.

I found that the essential oils of cinnamon and cloves possessed an antiseptic power, quite equal to that of kreosote, and that their aqueous solutions, when sulphated, were even superior to similar solutions of that agent.

One part of milk mingled with four parts of a saturated aqueous solution of the sulphated oil of cloves, remained after five days sweet and liquid, while another portion of the same milk became curdled and sour within twenty-four hours. Having on the 2nd day of July added two drops of oil of cinnamon to an ounce measure of fresh milk, it remained liquid on the 11th; and, though it finally coagulated, it continued free from bad taste or smell till September, although other portions of the same milk had become putrid. A half ounce of milk, to which a drop of sulphurous oil of turpentine had been added, remained free from coagulation at the end of two days, while another portion, containing five drops of pure oil of turpentine, became curdled and sour on the next day.

A number of pieces of meat were exposed in small wine glasses, with water impregnated with solutions of the various essential oils. Their antiseptic power seemed to be in the ratio of their acidity. The milder oils seemed to have comparatively little antiseptic power, unless associated with the sulphurous acid, which has long been known as an antiseptic.

In cutaneous diseases, and, perhaps, in the case of some ulcers, the employment of the sulphurous sulphated oils may be advantageous.

A respectable physician was of opinion that the sulphurous

sulphate of turpentine had a beneficial influence in the case of an obstinate tetter.

Possibly the presence of sulphurous acid may increase the power of oil of turpentine as an anthelmintic.

Pieces of corned meat hung up, after being bathed with an alcoholic solution of the sulphurous sulphated oil of turpentine, or with solutions of the sulphated oils of cloves or cinnamon, remained free from putridity at the end of several months. That imbued with cinnamon had a slight odour and taste of the oil.

I am led, therefore, to the impression that the antiseptic power is not peculiar to kreosote, but belongs to other acrid oils and principles, and especially to the oils of cinnamon and cloves.

The union of sulphuric acid with these oils appears to render them more soluble in water: whether any important change is effected in their medical qualities by the presence of the acid may be a question worthy of attention.

I have stated my reasons for considering the ammoniacal liquid, resulting from the ablution of the æthereal sulphurous sulphate of ætherine with ammonia, as partially composed of hyposulphuric acid. By adding to this ammoniacal liquid a quantity of sulphuric acid, sufficient to produce a strong odour of sulphurous acid, and then a portion of any of the essential oils; a combination ensued, as already described, between the oils and the sulphurous acid liberated by the sulphuric acid, so as to render them yellow and suffocating. The habitudes of cinnamon oil from cassia under these circumstances were peculiar. A quantity of it was dissolved, communicating to the liquid a reddish hue. The solution being evaporated, a gummy translucent reddish mass was obtained, which, by solution in alcohol, precipitated a quantity of salt, and being boiled nearly to dryness, redissolved in water and again evaporated, was resolved into a mass having the friability, consistency, and translucency of common rosin; but with a higher and more lively reddish colour. Its odour recalls, but faintly, that of cinnamon; its taste is bitter and disagreeable, yet recalling that of the oil from which it is derived. Its aqueous solution does not redden litmus; nor, when acidulated with nitric acid, does it yield a precipitate with nitrate of barytes.

Of this substance ten grains were exposed to the process above mentioned, for the detection of sulphuric acid, and were found to yield a precipitate of 6.5 grains of sulphate of barytes.

It may be worth while to mention, that in boiling the sul-

phated oils with nitric acid, compounds are formed finally, which resist the further action of the acid, and are only to be decomposed by the assistance of a nitrate and deflagration. I conjecture that these compounds will be found to merit classification as æthers formed by an oxacid of nitrogen.

One of my pupils, in examining one of the compounds thus generated, was, as he conceived, seriously affected by it, suffering next day as from an over dose of opium. He also conceived that a cat, to which a small quantity was given, was affected in like manner.

I had prepared an apparatus with the view of analysing accurately the various compounds above described or alluded to, by burning them in oxygen gas; when, by an enduring illness of my assistant, and subsequently my own indisposition, I was prevented from executing my intentions.

VIII. *On a Scale of Geometrical Equivalents for Engineering and other Purposes.* By Mr. CHARLES HOLTZAPFFEL, Associate of the Institution of Civil Engineers:

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE scale of geometrical equivalents is a particular combination of several of the scales of equal parts, which I have recently contrived and explained*: by means of this instrument, with the aid of a little arithmetic, a great variety of tedious calculations in constructive science generally, but more especially in engineering, may be performed with considerable correctness, by the simple inspection of scales, proportionals to the quantities under observation.

As regards drawing, the scales are shown to be only an extension and generalization of the common application of reduced scales of 1, 2, 3, &c. inches to the foot; and in their application to numbers or quantity, they are laid side by side with their zeros in contact, after the manner of thermometer scales, two of which are frequently engraved on the same instrument for comparison, so that we may read the height of the mercury either by the centigrade scale, freezing being called zero, and boiling 100; or by the Fahrenheit scale in common use in this country, the same space being divided into 180 degrees, freezing being then marked 32. We may thus transpose the French reading of the instrument into the English, or the reverse.

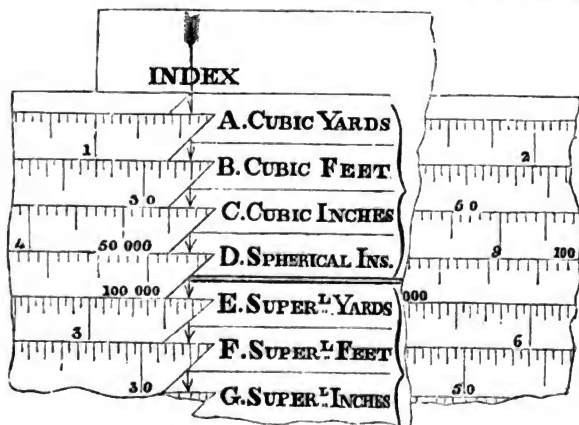
I have extended this latter application of scales to many

* A new System of Scales of Equal Parts, by C. Holtzapffel.

kinds of quantity, amongst others to the various measures and weights, as the linear measures, the cubic measures and measures of capacity, the superficial measures, and the weights of all denominations and all countries, any two of a kind admitting of comparison after the manner explained with the thermometer. The scales intended for drawing are graduated on *separate* slips of card, in order that they may be immediately applied to the drawing and to curves without the intervention of compasses, and at the same time the confusion arising from crowding together several lines of graduations on the same slip of wood or metal to avoid expense, is got rid of.

Card has been selected as the material from its economy, its flexibility, the distinctness of the black lines (ruled upon them in my new dividing engine,), the facility of writing or printing upon the card the titles and explanations, &c., but principally from the *identity of material* of the paper scales, and of the drawing paper, they are in consequence affected in the like degree by atmospheric influence, which has been experimentally proved*. The different series are ruled on cards of light colours for distinction.

The purpose of the present instrument, however, being different, the card is glued on a thin mahogany board six inches wide, so as to contain on one or both sides several lines of graduations, beginning from one line or zero; and any two of the scales may be readily compared, or the correspondence



* A New System, &c., pp. 1—9, and 43.

of the whole series may be observed at one view, by the assistance of a particular kind of square represented in the wood-cut. It consists of two slips of card, connected at the upper and lower edges by two slips of wood glued between them, so as to make a kind of slider or ferrule.

The first idea was to cut the edge of the card square; but a better mode subsequently suggested itself, namely, to draw a line across the index, and then to serrate the edge, so that the several parts of the line might serve for the more accurate appreciation and subdivision of the graduations by the eye. At the same time, the card index receives the titles of the scales A, B, C, &c. and there is therefore no necessity to distress the eye by running continually to the end of the instrument to ascertain the same, as the index gives all the particulars called for, in immediate contiguity.

The 20 lines of graduations selected for one side of the present instrument are arranged as follows:

A. Cubic yards	}	Cubic measures.
B. Cubic feet		
C. Cubic inches		
D. Spherical inches		
E. Superficial yards	}	Areas and columns, one foot high or deep.
F. Superficial feet		
G. Superficial inches		
H. Circular inches		
I. Imperial gallons	}	Measures of capacity.
J. Bushels		
K. Barrels		
L. Tuns		
M. Pounds troy	}	Weights estimated in water.
N. Pounds avoirdupois		
O. Cwts.		
P. Tuns		
Q. French lineal mètres	}	French metrical system.
R. French cubic mètres		
S. French super ^l mètres		
T. French kilogrammes.		

All these scales are graduated to one common standard, namely, the unit described in the pamphlet*, one tenth of an inch English representing one cubic foot English: all the others are multiples and submultiples, proportionate to the values of the several measures.

The line B, therefore, the basis of the instrument, is a line of inches and tenths, marked 10, 20, 30, &c. Now as the cubic yard contains 27 cubic feet, the unit of the line A is $B \times 27$ or 2·7 inches decimally subdivided.

The line C for cubic inches is so proportioned that 1728 of its nominal divisions, (the number of cubic inches contained in one cubic foot,) are equivalent to one tenth of an inch; but as we could not graduate such a scale, nor employ it when done, our purpose is equally well served by the numerals annexed to the divisions.

Ten inches denotes 100 cubic feet on B, that space must also denote 1728×100 or 172,800 cubic inches on C, 10 inches is therefore divided into 17 great divisions, numbered respectively 10,000, 2, 3, 4, 50,000, 6, 7, 8, 9, 100,000, and so on, each fifth numerical being the true number of cubic inches, the ciphers being omitted in the intermediate places to avoid confusion.

The line D for spherical inches is C multiplied by ·5236 the constant multiplier for giving the value of the sphere enclosed in any given cube, or as expressed at the end of the scale itself $C \times \cdot 5236 = 3\cdot030$. In this simple manner all the values are worked out and graduated, the formula for constructing each being marked at the terminations of the several lines.

It only remains to observe that the group for superficial measures is calculated for areas and columns of the common height of one foot; the group for weights, which refer to water, from the cubic foot of water weighing 62·5 pounds avoirdupois, therefore 10 inches represents 6250 pounds avoirdupois; the group for measures of capacity, from the gallon being equivalent to 10 pounds of water, and so on.

The scales of the instrument being therefore proportionals as explained, we may read off in groups the value of any one measure in any other: it is desired, for example, to know all the equivalent values of 33 cubic feet, expressed upon the scale, (see diagram). Set the index to 33 on B. cubic feet, and at one view the several answers appear, namely, on A. 1·22 cubic yards, on B. 33 cubic feet, on C. 57,000 cubic inches, (the cube root of which from the tables or 38 to 39 will be the side in inches of an equal cube,) on D. we read 109,000 spherical inches, (the cube root of which 48 nearly is the diameter in inches of a sphere containing 33 cubic feet,) on E. 3·67 square yards, on F. 33 square feet, on G. 4750 square inches, (each area being supposed to be the base of a column one foot high, and the square root of any of these will give the side of an equal square column of the same height); on H. we read 6050 circular inches, the square root of which is the diameter in inches of a circular column or cylinder one foot high, (also containing 33 cubic feet) on I. 206 gallons, on J. 25·75 bushels, on K. 5·72 barrels, on L. ·818 liquid tuns, on M. 2500 pounds troy, that being the weight of 33 cubic feet of water,

on N. 2060 pounds avoirdupois, (multiplying either of these by the specific gravity of any substance, gives the weight of each, or it may be done by inspection of the scales provided for that purpose p. 24—30,) on O. we read 18·4 cwts., on P. ·92 tuns, on R. ·932 cubic *mètres*, on S. 3·06 superficial *mètres*, as before extending to the height of one foot, and finally on T. 932 *kilogrammes*, as the weight of 33 English cubic feet of water.

It is not at all likely that the whole of these comparative values would be wanted in any one inquiry, but all the measures are of frequent occurrence in calculations. To render the reading as simple as possible the numerals denote the true values throughout, so that no reductions nor changes have to be made, unless it be the calling 1, either 10, 100, 1000, &c. which is common to all decimal scales: it need scarcely be said that when the value of any one scale is thus altered, the same must be done with all employed at the same time. It will be found the most convenient first to take down the numbers denoted on the scale, and then to seek the true place for the decimal point.

The results given in the general example show the varied nature of the transpositions which the scale effects: these need not be extended by way of illustration; but we may also read and resolve decimals, and perform many of the calculations in engineering, &c. which would otherwise require the employment of two or more constant multipliers.

Required the value of the decimal ·231 of a cubic foot, in cubic inches. Employ B. and C. The answer is 399·2 inches.

Convert the decimal ·987 of a pound troy into the decimal of a kilogramme. Employ N. and T. The answer is ·3685.

What fraction of a liquid tun of water is $7\frac{1}{4}$ cwts. of the same? Employ O. and L. Answer ·322.

The diameter of a sphere of water containing 500 gallons? Seek 500 in I. and take the cube root of the number obtained in D (from the tables). Answer 263,900, the cube root of which is 64 inches nearly.

Required the side of an equal cube. Read 64 the diameter of the given sphere in D, and the answer 33·5 will be found in C; the two lines C. and D. performing the multiplication and division by ·5236.

And in the like manner the lines G. and H. effect the multiplication and division by ·7854. Required the diameter of a column 45 feet high to contain 790 gallons. Divide the quantity by the height in feet for the contents of one foot, for which height the scales are calculated; answer 17·55 gallons: seek that number in I. and take the square root of the number found in H., namely, 515·4, as the answer, or squared, 23 inches nearly, as the diameter of the column.

Required the contents of a cooling floor for a brewery 19 yards long by 20 yards wide, covered to the depth of 9 inches, in cubic feet, barrels, and tuns avoirdupois. By arithmetic $19 \times 20 = 380$, the area of the floor in square yards; deduct $\frac{1}{4}$ th, 9 inches being $\frac{3}{4}$ ths of 1 foot, for which depth the scales are calculated, 380 less $\frac{1}{4}$ th = 285.

Seek 285 in E. and read the answers in B. 2565 cubic feet, in K. 445·2 barrels, and in P. 71·56 tuns.

Required further how many times this quantity would be required to

fill a vat 30 feet diameter and 40 feet high, and also the pressure against the side of the vat at the bottom, and 10, 20, and 30 feet below the surface. To find the contents of the vat square the diameter, and multiply it by the depth, (by arithmetic) $30 \times 30 \times 40 = 36,000$. Seek 36,000 in H. and the contents will appear 28,280 in G. (Thus the scales G. and H. although representing inches in the general scheme, have given the product of $36,000 \times .7854 = 28,280$ cubic feet.) Seek 28,280 in B. the proper scale for cubic feet, and the answers 4910 barrels will be found in K. and 701 liquid tuns in L.

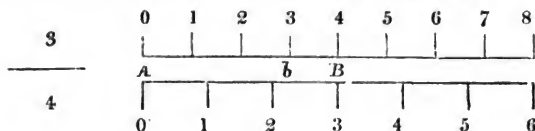
It only remains to divide the entire contents of the vat 28,280 by the contents of the floor 2565, the quotient 11 nearly will be seen on inspection as the number of brewings required.

To find the pressure against the bottom of the vat, &c. seek the depths 40, 30, 20, and 10, in G, and the pressures will be read respectively in M. 17.4, 13.1, 8.7, 4.35 pounds avoirdupois.

The scales on the other side of the instrument are for the quadratic and cubic relations of quantities.

Scales are made up of two parts, the spaces which are geometrical quantities or extents, and the numerals which are arithmetical quantities. So likewise do they admit of two modes of application, first to the measurement of spaces by means of their spaces, as in making reduced and enlarged drawings, *proportional spaces* being then marked by *equal numerals*;—and secondly, to the measurement of numbers, as in the slide rule, the thermometer scales, &c., *proportional numbers* in this case being marked by *equal spaces*.

This will be rendered very clear by an example. Required $\frac{3}{4}$ of a line A B, and $\frac{3}{4}$ of the number 8. Select two scales in the proportion of the terms of the fraction, namely,



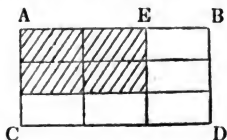
The spaces of the three scale are each three tenths of an inch, and those in the four scale four tenths.

The line A B equals 3 spaces of the denominator scale, but the three spaces of the numerator scale A b are only $\frac{3}{4}$ of A B, A B being divided into 4 parts on the three scale; therefore in drawing, we measure the object with the denominator scale, and we draw from corresponding numbers on the numerator. But if we desire to know $\frac{3}{4}$ ths of 8, we find that as regards the numerals on the scales matters are reversed, the greater spaces require fewer figures in the same extent, or they are numerically of smaller value; therefore to read $\frac{3}{4}$ of 8 by the above scales, we seek the given number in the numerator, and we find the reply 6 over against it on the denominator scale.

The above application of scales is true of all ratios expressed as vulgar fractions, decimal or others, as $\frac{7}{8}$, $\frac{11}{10}$, $\frac{23}{4}$, $\frac{256}{144}$, $\frac{709}{230}$, it being essential the scales should be 7 and 8, —11 and 9, —23 and 4—2·236 and 1·414 times any unit, and that they are employed as explained. The three first fractions are comprehended in the scales prepared for lines or drawings, which are 1, 2, 3, 4 to 25 inches long, similarly divided and figured*. As we obtain from them all the distinct proportions that may be expressed as vulgar fractions in terms not exceeding 25, (which are given in the pamphlet both as fractions and decimals, and arranged in the order of magnitude) they offer great facility for making reduced or enlarged copies of drawings, models, &c. after various manners, many examples being given, and they likewise serve for working numerical ratios of simple proportion. The two latter or the decimal fractions would result from the employment, after the same manner, of two other series of scales for the areas of superficies and the contents of solids, the units of which are respectively the square and cube roots of the numbers 1 to 10.

These may need a little more explanation. Required $\frac{4}{9}$ ths the size of a given area A B C D.

“The parallelogram A B C D is divided into 9 equal and proportional parallelograms, by the division of each of its sides into three parts. If, however, we only reckon two parts each way, we find them to include but four out of the nine equal areas. The square root of 4 is 2, and the square root of 9 is 3, therefore the areas being as 4 to 9, the sides are as 2 or 3, or as the square roots of the former, which was to be shown. This would be equally true if the scales were applied in the inverse order, and of any two; also if the figures were complex; for a semicircle, or equilateral triangle on A B, would have for the new diameter or side A E, and so with every part of which the figure might be made up.”



The scales for solids admit of the same explanation.

There are 24 lines of graduations on the other face of the scale, (or it is somewhat more convenient to have them as two distinct instruments,) namely, the scales for areas $\frac{1}{10}$, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10 and 100, and the same number for solids. The first and last are for those cases in which the ratio of enlargement or diminution cannot be expressed in fractions not exceeding 10 in either term, which will be referred to hereafter.

* A New System, &c. pp. 16—23.

From the name selected for the instrument, namely, the scale for geometrical equivalents, it may be seen it is principally designed for the alteration of *known* forms and quantities; it therefore remains to show this application to any solid of known contents, in any ratio, and in any prescribed manner.

1. When one dimension only is altered, it will be in the direct proportion of the ratio, the original figure being considered as unity.

2. When two dimensions are altered, it will be in the duplicate ratio of the two, or as the square root of the ratio.

3. When all three dimensions are altered, it will be in the triplicate ratio of the sides, or as the cube root of the ratio.

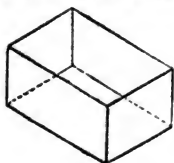
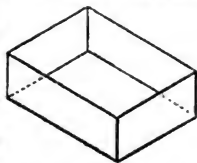
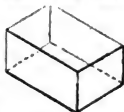
For example, a given vessel measures 6 feet long, 4 feet wide, and 3 deep, its contents are therefore $6 \times 4 \times 3 = 72$ cubic feet. Required the dimensions of other vessels to contain 180 cubic feet, according to each of the modes. The new vessels will be to the original or unity as 180 to 72 or $\frac{180}{72}$ times the size, which reduced to its lowest terms is $\frac{5}{2}$ or $2\frac{1}{2}$ times as great.

1. To enlarge the one dimension only, say the depth, multiply it by the ratio $\frac{5}{2}$, $3 \times \frac{5}{2} = 7\frac{1}{2}$, the measure of the new depth, and $6 \times 4 \times 7\frac{1}{2} = 180$, the new contents.

2. A new vessel to be of the same depth, but to have its area enlarged $2\frac{1}{2}$ times. The new sides will be found by multiplying the given sides by the square root of the ratio, or $\sqrt{\frac{5}{2}}$ or $1\frac{2256}{4112}$; this by any of the modes of arithmetic, or by logarithms, would take some time; whereas by the scales, the 5 quadratic scale having for its unit 2.236 inches, and the 2 scale 1.4142 inches, (the square roots of 5 and 2,) they represent the above fraction, and the application is precisely the same as seeking $\frac{5}{2}$ ths of 8 already explained: set the index to 6, the given side on the numerator scale 5, and the new side is found in the denominator scale 2, namely, 9.48; and do the same for the other side 4 feet, which comes out 6.32. I have never taken the trouble to obtain these quantities otherwise, and the multiplication of the 3 sides one into the other gives the new contents, 179.74, whereas it ought really to be 180, so that the error only amounts to about the 720th part of the whole quantity.

3. The vessel to contain $2\frac{1}{2}$ times as much as the given one $6 \times 4 \times 3$, enlarged in each of its dimensions, will be found by multiplying each of the sides by the cube root of the ratio, or $\sqrt[3]{\frac{5}{2}}$. Therefore, seek 6, 5, 3 in the 5 cubic scale, and the new sides will appear respectively over against them in the denominator scale 2, namely, 8.16, 5.43, 4.08, which multiplied into one another for the new contents give 180.779, about the 240th part too much, a result sufficiently near for most practical cases.

Of course, what is true of the two scales em-



ployed, will be true of any two, and the method is applicable to all solids, of all forms and sections. It is desired to increase the contents of a pan or vessel $2\frac{1}{2}$ times, the same as explained for the cubical vessel. Draw several ordinates across the figure.

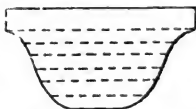
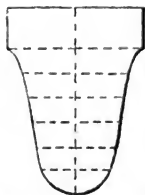
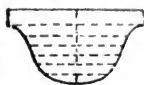
1. The vessel being altered in height only, the diameters remaining constant, increase the distance between the ordinates $2\frac{1}{2}$ times, whence the second form immediately beneath would result.

2. The figure being altered in diameter, the depth remaining constant, the lengths of the ordinates must be increased $\sqrt[2]{2\frac{1}{2}}$ times by the quadratic scales: the new figure would be of this form.

3. The three dimensions being altered, the space between the ordinates, as well as their lengths, must be multiplied by $\sqrt[3]{2\frac{1}{2}}$ by means of the cubic scales 5 and 2; this new figure would be produced, or a copy of the original in the same proportions as to height and diameter as the original: and further, should it be desired to mix the two modes, that is to alter both the general contents, and one of the measures in any defined ratio, for example, to make a new vessel as before containing $2\frac{1}{2}$ times as much, but of only half the height, we must thus learn the ratio. Half the height with the same diameters would give half the contents; the increase of the diameters by the quadratic scales must therefore be doubled by employing the quadratic scale $\frac{4}{1}$, $\frac{1}{2} \times \frac{4}{1}$ being equal to $\frac{2}{1}$ the ratio required for the new contents.

We have therefore complete command over the capacity of all vessels, under all proportions of general contents, and specific variations of form. The truth of this method admits of rigid demonstration, if we consider the ordinates to divide the figure into so many zones, or as regards the section into trapezoids and rectangles, and that the curved line running through them is superseded by short right lines. Of course, the more ordinates that are used the more nearly true will be the result. The circle would by this treatment become an ellipse, which may be taken as a further proof of the correctness of the result, as to find the area of an ellipse the two diameters are multiplied into each other, and the product by .7854.

When vessels of complex form are constructed as retorts, boilers, pans, stills, tanks, &c. it would be desirable to retain drawings of them, with the several measures written upon them, and also a memorandum of the cubic contents obtained either by calculation or experiment, as by means of the scales these data may be employed for obtaining the new contents,



and dimensions of similar vessels, under all varieties of size and form with considerable accuracy, whereas the complexity of such calculations often cause them to be neglected, leaving the results to the unassisted judgement, or in other words to be guessed at.

The scale of geometric equivalents being, as before adverted to, rather an instrument for transposition, than calculation in the strict sense of the word, it will be found desirable to estimate the dimensions of certain known forms of given areas for reference; a few have been tabulated.

	Containing 100 Superficial Feet.	Containing 1000 Superficial Feet.
Diameter of a circle.....	11·284	35·682
Side of a square	10·	31·623
Side of a hexagon	6·204	19·619
Side of an octagon	4·551	14·392
Side of a decagon.....	3·605	11·400
Side of a dodecagon.....	2·989	9·451

They would be employed in the following manner:

Required the diameter of a circular gasometer to contain 60,000 cubic feet when filled to the height of 20 feet. 1000 cubic feet would be contained in a circle 35·68 feet diameter, 1 foot high; the height being 20 feet, would necessarily contain 20,000 feet, therefore the proportion would be $\frac{60,000}{20,000}$ or $\sqrt[3]{\frac{3}{1}}$; seek 35·68, the given diameter in the 3 quadratic scale, and the new quantity 61·9 will be found in the denominator scale.

Required the side of a hexagonal tank to contain 3600 cubic feet when filled 5 feet high. The side of a hexagon containing 100 feet is 6·2, the ratio is therefore $\frac{3600}{100}$ or $\sqrt[3]{\frac{36}{1}} \times 6·2 = 16·45$: this quantity obtained from the quadratic scales 7 and 1 would be one 35th too small, and might be corrected to that amount by a second process if required; and if in the like manner it were required to multiply any quantity by the factor $\frac{27}{1}$, we might arrive at the result at twice as $\frac{2}{1} \times \frac{3}{1} = \frac{27}{1}$, and so on.

It will, however, be found in general the most convenient to bring the ratio within the series of fractions expressed by the numbers 1 to 10, beginning with $\frac{1}{10}$ th, and ending with $\frac{1}{1}$, or ten times, which are tabulated; but we may take cognizance of ratios very much larger and smaller after the following manner.

We may employ any two terms indifferently provided they are in the same proportion; for example, the terms 1 and 2,—2 and 4,—3 and 6,—4 and 8, are each as one to two: their squares are 1 and 4,—4 and 16,—9 and 36,—16 and 64, or as 1 to 4 throughout; it follows therefore that $\frac{1}{10}$ being the tenth part of 1, the 20th part of 2, the 30th of 3, and so on,

we obtain the proportion 10, 20, 30, 40 times, or $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{30}$, $\frac{1}{40}$ th part by the joint employment of the $\frac{1}{10}$ th and the 1, 2, 3, 4 scales, &c.

Again, the square root of 100 is 10, of 200, 14.142, of 300, 17.320, or ten times the square roots of 1, 2, 3; we may therefore call these scales 100, 200, 300, by simply multiplying the numerals by 10, so that to obtain $\sqrt[3]{\frac{200}{7}}$ of any quantity, seek it in 9, and the answer in 7, which, supposing it to be 5.314, must be read 53.14; and lastly, $\frac{1}{10}$ being the thousandth part of 100, by employing the $\frac{1}{10}$ th line on the one hand, and the 1, 2, 3, 4, 5 lines, (multiplied by 10,) on the other, we obtain the $\frac{1.000}{1}$, $\frac{2.000}{1}$, $\frac{3.000}{1}$, and so on: these matters are, however, of great simplicity to those at all accustomed to the employment of decimals.

An example will better explain the utility of this arrangement.

A mine 650 feet deep has to be drained by means of a pump 17 inches diameter: required first the total contents of the entire column of water in cubic feet, gallons and tuns weight, and also the pressure on each square inch of the column; and the requisite diameter of a steam cylinder, working under the effective pressure of 12 pounds on the inch which shall balance the column in the mine.

17 the diameter of the pump or column squared and multiplied by the depth or $17^2 \times 650 = 187850$, seek that number in G. (square inches) and read in B. 1304 cubic feet, in I. 8140 gallons, and in L. 3231 tons avoirdupois, as the contents of the entire column.

For the pressure on each square inch of the pump, seek the depth 650 feet in G, and against it 282.5 will be found in M. The pressure per inch on the pump cylinder being 282.5 pounds, and the proposed effective pressure of the steam being 12 pounds per inch, the ratio in which the cylinder should be altered is $\sqrt[3]{\frac{282.5}{12}}$.

The required result, and the others presented at the same view, are extracted in three columns: the first column shows the real titles of the quadratic scales,—the second the assumed titles, or the former multiplied by 3 (300 being nearest to 282.5); and the third column the diameters of the equivalent cylinders under each different pressure, the given diameter of the pump having been sought in the 300 column.

Scales	100,	1,	2,	3,	4,	5,	6,	7,	8,	9,	10.
Pressures	300,	3,	6,	9,	12,	15,	18,	21,	24,	27,	30.
Diameters	17,	170,	121,	98,	85,	76,	70,	64,	60,	57,	54.

The nearest whole numbers having been taken. The truth of this will be apparent $17^2 \times 300$ being 86700, and $85^2 \times 12$ being likewise 86700.

But as a comprehensive example, suppose it is desired to enlarge the drawings of a steam-engine, the new one to be as 5 to 4.

The contents of all the vessels, as the cylinder, condenser, air-pump, &c. will be increased by the cubic scales 5 and 4.

The areas of the valves, passages, pipes, and the compli-

cated surface of the internal flues or fire surface of the boiler, by the quadratic scales 5 and 4.

The new and old cylinders being in simple height as 15 to 14, the lengths of the beam, rods, arms, &c. would be extended by the scales for lines 15 and 14, and so on. If the proportions of the vessels were altered, the application of the scales would be modified as already shown by the diagrams.

This paper might have been easily extended by other examples of many kinds, but I trust that enough has been said to explain the principle and general method employed, that alone being required in this place.

The scale of geometrical equivalents in some respects resembles the slide rule, but it possesses certain advantages.

1st. It is easier to graduate scales of equal parts than those in which the divisions are unequal.

2nd. It is easier to subdivide equal divisions by the eye than unequal ones, and they admit of the employment of the vernier if required.

3rd. The results are obtained in groups, whereas each separate question requires a distinct setting with the slide rule.

4th. In using the slide rule we are subject to two sources of error, imperfect setting as to the gauge-point, and imperfect reading off in the quantities. From the construction of the scale of equivalents, as the scales all begin from one common line, the setting can never be disturbed. It is therefore only liable to the single error of reading off, the settings being constant.

5th. In order to obtain the most accurate results it is occasionally desirable to double or halve the quantities, so as to get as far from the zero of the scales as possible, whereby the error of reading will be proportionably diminished. The very nature of the slide rule limits this application, as sometimes only a third part or less of the scales remain in contact.

6th. But the most serious drawback to the slide rule is the constant practice that is required for using it with facility, arising principally from the difficulty of assigning the true value to the answers, whether units, tens, hundreds, thousands, &c., and the constant reference to the tables of the gauge-points.

Now, as in all cases the true values are expressed upon the scales of the new instrument, and their particular names denote at the same time the functions and relationships of the several scales, both inconveniences are in a great degree, if not entirely, obviated.

7th. It is true, the slide rule may be employed for any arbitrary setting, whereas the present instrument is in a measure

limited to certain constant multipliers; but the combined result of some three or four of these are sometimes concerned in giving a single solution on the scale of geometric equivalents, and that in a novel and curious manner.

The multipliers selected possess an extensive range, and include the elements of most of the calculations likely to be called for; the mode to be pursued can scarcely be mistaken, as the simple names of the functions serve to indicate the proceeding in each case; and when a little arithmetic and the tables of squares and cubes are brought in to assist, the powers of the contrivance are exceedingly increased. We exchange the tediousness of calculation and the errors which creep in, notwithstanding every care, for the certainty of the relations of the lines; subject however to the error of reading, which will not in general be found an inadmissible quantity; it rarely equals the one hundredth part of the totals, and is sometimes within the thousandth part of the same. The answers given in this paper were obtained from the instrument alone: their proof by calculation, (in which the formulæ on the scales will assist,) will show the degree of reliance that may be placed on the method.

Of course such scales might be adapted to a variety of other investigations, and they would be made to any given data.

In order to lessen the expense of the present instrument, I propose to graduate the scales on copper-plate* and to print from them. I should therefore feel obliged by any suggestions calculated to improve the arrangement proposed previously to so doing.

I remain, Sir, your most obedient servant,

64, Charing Cross, London,
June 6th 1838.

C. HOLTZAPFFEL.

IX. *On the primary Forces of Electricity.* By RICHARD LAMING, Esq., M.R.C.S.

[Continued from p. 498.]

32. **W**E may now apply the principle of compensation to some of the leading phænomena attendant on the accumulation of free electricity in rarified air. As the quantities susceptible of being accumulated in conductors under atmospheric compensation have been shown, both theoretically and experimentally, to vary in the simple ratio of the density of the air, the quantity that is communicable from one body

*As explained in the pamphlet, the error of contraction after printing is immaterial with the proportional scales; it is only inadmissible in such as demand an agreement with the standard measure, pp. 6 and 7.

in plenum, to another supposed to be insulated in an absolute *vacuum*, is equal to nothing.

33. From this it evidently follows, first, that when a conductor containing a plus charge be *partly* inclosed in a receiver, and the air within the latter is gradually withdrawn, the free electricity will progressively recede to those parts of the conductor which are external to the receiver; for by the theory the several parts of a conductor can never differ in intensity (28.); which in this case they would do unless the charge requiring compensation, and the air by which it is compensated (*both* within the receiver) diminished in the same ratio.

34. In the second place we learn that by reducing the density of the air when a charged conductor is insulated *wholly* within the receiver, we virtually increase the quantity of electricity to be compensated by an assumed unit of air; thus causing the force of attraction between that electricity and its compensator to be increased. Now the intensities being as the square of the quantities directly, and the quantities virtually as the densities of the air directly, the electrical intensity of any given charge is as the square of the atmospheric density inversely; and this is the conclusion Mr. Harris has arrived at by experimenting in various ways*.

35. We are thus enabled to understand why a conducting body highly charged and insulated under a receiver gradually becomes discharged as the vacuum becomes more perfect; a fact which has been long known, but until now never explained. It is usual to ascribe it to diminished resistance by the atmospheric air; but the phenomenon may be made to take place equally when the pressure remains undiminished; which has been proved by first abstracting part of the air from a receiver and then heating the residuum; under such circumstances it was found that, however the temperature, and with it of course the pressure, might be changed, so long as the quantity of free electricity remained constant, its intensity varied as the square of the density or quantity of the air, inversely†.

36. The phenomena which characterize the deflection of the pith-balls, straws, gold-leaves, or other moveable parts of diverging electroscopes, are rendered intelligible by the application of our principles. Suppose the compensation of a plus body B, free to move, to devolve entirely on two other uninsulated fixed bodies, A A', placed on either side and at equal distances from it; B will have an equal tendency to

* Phil. Trans. 1834, part ii.

† *Ibid.*, p. 228.

move in two directions, and consequently remain stationary ; now if B be divided into two parts in a plane at right angles to a right line passing through the axis of A A', the two parts will instantly separate.

37. Just so it is in experiment ; two balls, for instance, charged with free electricity and in contact, are virtually one conductor ; compensated, when freely insulated in the atmosphere, solely by the air. While the two balls are in contact the compensating atmosphere of neither can be perfect spheres ; and hence it must extend to a greater distance from the surface than if it wholly surrounded each of the bodies individually ; and as by Coulomb's law the force of attraction is much greater as the distance is less, the distant particles of compensating air will be brought near to the attracting surfaces, and thus by intervening between the balls separate them from each other.

38. Coulomb has ascertained by experiment that the force with which two bodies *equally charged*, thus apparently repel one another, varies as the square of the distance inversely ; which is quite in accordance with the principles we are examining ; for before two bodies can come together the intervening air must be removed, and this we have shown to be held against *each* body with a force varying as the square of the distance inversely ; hence the apparent repulsion of two such bodies will be equal to the sum of the individual forces in each body, but this addition leaves the ratio unaffected.

39. In the preceding case the charges of the two balls being equal their compensating atmospheres were of the same extent ; but we may so dispose the free electricity on two balls that their charges shall be unequal, and then the radii of their compensating atmospheres will differ also. Under such circumstances of course the law of Coulomb will not express the apparent repulsion at *all* the distances ; this theoretical conclusion Mr. Harris has arrived at by the induction of a series of nice experiments made with his very ingenious bifile balance*.

40. But the influence of the major attraction in causing the divergence of an electroscope has always to encounter a retarding force in the *reaction of the instrument* ; and however minute this latter force may be, it must occasion the two compensating atmospheres in some measure to intersect one another ; with this in recollection we shall easily perceive that one of two charged balls may have so much of its free electricity abstracted as to enter *itself* within the compensating atmosphere of its associate, in which event of course instead

* Phil. Trans. 1836, p. 430 *et seq.*

of receding from each other, the two balls will, as shown by Mr. Harris, be brought together*.

41. We may next inquire into the sufficiency of the new theory to explain the various circumstances under which discharges of free electricity may be made to take place between plus conductors and their compensating bodies.

As action and re-action are in all cases equal, it is evident that the minus common matter in a negatively electrical body must as forcibly attract the free electricity in a plus body as it is itself attracted by it; and therefore were the cases parallel in other respects also, the phenomena of electrical attraction and electrical discharges would at all times be simultaneous.

42. In the case of electrical attractions we certainly have a retarding force in the gravity of one or both of the attracted bodies, and frequently in the gravity of materials to which they are attached; and this retarding force may be sufficiently great to overcome the attractive force at any distance, however small. On the other hand, either one or both of the attracted bodies may be so free to move as to oppose a retarding force indefinitely minute; and then they will be brought together through a distance almost, though never quite, so great as that through which under the circumstances their compensating influence extends.

43. Now unless there were a retarding force to electrical discharges, these would occur at the extreme distance at which the major attraction acts, or in other words, at which compensation becomes established; and thus the discharging distances being greater than the distances at which visible attraction is effected, the latter phenomenon never could take place. It is obvious therefore that electrical discharges are restrained by some retarding force; and this is supplied in the minor attraction acting between the free electricity in the plus body, and the electrical equivalent which is natural to it.

44. Since then the minor electrical attraction in every plus conductor is a retarding force to the discharge of its free electricity, it will act with less effect upon any particular electrical atom as it is further removed from the general mass; and therefore a plus charge may be drawn off with a less amount of major attraction, and consequently at greater distances, from projecting points and angles than from any other surfaces. Hence the great freedom with which electricity issues from a point to its compensating atmosphere; and hence also the escape of the electrical spark towards a conductor, at greater distances through the compensating at-

* Phil. Trans. 1836, p. 431.

mosphere as the part of the charged body to which the conductor is presented is smaller and more prominent.

45. The form of the conductor, or of that part of a conductor from which the plus charge is to be abstracted, being given, then if an unit of quantity of free electricity be retarded in it by an unit of minor force, two units of quantity should be retarded by two units of force, three quantities by three units of force; the retarding force thus continually increasing in the simple ratio of the quantities of free electricity. Consequently the discharging distances from any given surface should vary in the same ratio; for, as we have already seen, (10) the intensity of abstracting, or major force being constant, the distances at which it acts vary in this ratio.

That this is the ratio observed by electrical discharges under all circumstances may be proved by an appeal to facts; and I think the proof will be received as an additional evidence of the minor electrical force.

46. Electrical discharges, as they occur between conductors compensated in the open atmosphere, have been examined by Mr. Harris; whose experiments on the subject decide most unequivocally, that under such circumstances at least, and whether the conductors be connected with Leyden surfaces or not, "the quantities of electricity requisite to produce a discharge vary with the distances directly*."

47. Mr. Harris has also ascertained with his usual accuracy, that in a rarified atmosphere "the distances through which a given accumulation could discharge, varied in an inverse simple ratio of the density of the air. Thus in air of one half the density the discharge occurred at twice the distance†." Now by reducing the density of a compensating atmosphere to one half, we virtually double the charge to be compensated by the residuum; in this case also we therefore see that the discharging distances are as the quantities of free electricity.

48. The same acute philosopher has further proved, by a very ingenious and exact mode of experimenting, that provided the densities or quantities of air in a receiver remain unchanged, the temperatures may take a very wide range without affecting the discharging distance of a given accumulation of free electricity; in this case again the discharging distances are therefore as the quantities of free electricity.

49. The very great distances at which electrical discharges may be made to take place in vessels nearly exhausted of air has induced a general belief that, in the language of Dr. Ure, "electricity is confined to the surface of bodies by a species

* Phil. Trans. 1834, p. 225.

† *Ibid.*, p. 229.

of mechanical pressure which air exercises." In the valuable magazine of facts to which I have found occasion so frequently to refer, there is enough said to carry with it a conviction that Mr. Harris is by no means satisfied of the truth of the supposition; indeed he has in one place actually proved that the resistance to electrical discharges does *not* vary as the atmospheric pressure; it would appear however that he is unconscious of his achievement; for in other places we find him insisting on the opposite conclusion as the legitimate induction of his experiments generally.

50. The proof to which I allude is furnished by a series of experiments, in which the temperature of a *given quantity of air* inclosed in a receiver was made to vary between 50 and 300 degrees of Fahrenheit, without "in the least" affecting the discharging distances of the electrical accumulations; notwithstanding the atmospheric pressure at different times of course differed enormously*. In these experiments, as in all the preceding, the discharging distances were as the *quantities of compensating air*, and therefore by our principles as the quantities of free electricity; that is to say, as the abstracting force minus the retarding force of the minor electrical attraction.

51. I have made many experiments on the discharging distances of free electricity in receivers filled with rarified air, without finding anything which offers the least evidence of electricity being retained on the surface of bodies by reason of atmospheric pressure; but on the contrary every evidence which the investigation was capable of affording has been obtained in favour of the principles generally on which this theory is based. For instance, let the density of the air, or its pressure, have been what it may, the discharging distances have invariably increased in the simple ratio of the quantities, until the sides of the receiver and the external air have by their comparative proximity interfered with the ratio by assuming part of the compensation of the charge.

In the following experiments, which are adduced as examples of such an interference, the quantities of electricity were estimated by the unit jar; the discharges were made between two brass balls $1\frac{3}{8}$ ths of an inch in diameter, placed in the axis of a receiver, more or less exhausted, about $4\frac{1}{4}$ inches in diameter and 13 inches high; and the balls communicated, respectively, with the opposite conductors of a Leyden jar exposing about two square feet of coating.

* Phil. Trans. 1834, p. 229, 230.

Table F.

Discharging Distances in Inches.	Comparative Quantities.	Same by Cal- culation.
1	35	
2	70	70
3	106	105
4	140	140
5	174	175
6	199	210

Table G.

Discharging Distances in Inches.	Comparative Quantities.	Same by Cal- culation.
1	12	
2	24	24
3	36	36
4	48	48
5	60	60
6	71	72
7	77	84
8	81	96
9	86	108
10	92	120
11	99	132
12	107	144
13	115	156
14	118	168
15	125	180
16	126	192
17	140	204
18	141	216
19	144	228

Table H.

Discharging Distances in Inches.	Comparative Quantities.	Same by Cal- culation.
1	42	
2	56	84
3	60	126
4	66	168

52. In these experiments the true ratio may be observed until the distance between the balls bears a certain relation to their distance from the external and denser atmosphere; after which, as appears by table G, the quantities of electricity re-

quired by that ratio become less by certain decrements, increasing for some distance nearly in arithmetical progression.

53. That the comparative contiguity of the sides of the receiver really produced the apparent discrepancies, was verified in two different manners; first, by substituting for the balls between which the discharges were taken others of smaller size, by which the true ratio was extended to a greater distance; and secondly, by replacing the receiver by others, first of greater, and then of lesser dimensions, in which case the extension of the true ratio became also greater and less respectively.

54. It would appear from this that in the discharge of a Leyden jar through long tubes of glass filled with rarified air, although the charge emanating from the ball connected with the positive coating really enters the ball proceeding from the negative coating, the free electricity is first attracted and passed onward, by induction, by the more contiguous bodies; and this view of the case not only explains the sensible passage of electricity along the surface of the glass, as frequently observed in experiments of this kind, but it makes clear also certain other phænomena of a similar nature which otherwise would not be well understood.

55. For example, a quantity of free electricity, which under ordinary circumstances may be discharged through the substance of a conducting body, perhaps heating it to redness, or even fusing it, will if the experiment be made in rarified air pass only along its surface*; a phænomenon which we may refer to the inadequate resistance by the minor force *in the conductor* to the intensity of the major force acting in the opposite direction, and now made very great by reason of the reduced quantity of its contiguous compensating atmosphere.

56. Let us in the next place examine our principles with reference to the phænomena which ensue when *both* the plus and the compensating conductors are in a state of insulation. Already it has been explained, that such bodies may compensate free electricity, by becoming themselves positively electrical (12.); and we have had abundant opportunity of contemplating such an induced electrical condition as we find it impressed on the insulating atmosphere in which experiments for the most part are conducted: we shall do well to consider also the same condition as it may be induced in insulated solids.

* Phil. Trans. 1834, p. 242, and Singer's Elements of Electricity, p. 63.

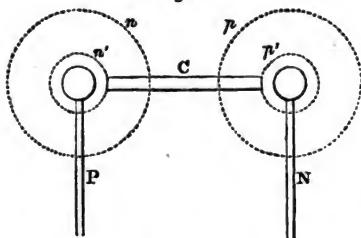
57. Imagine a plane surface A, to be insulated and charged with free electricity; its compensator will be the contiguous atmosphere, this being an insulator acquiring in consequence a plus condition. Suppose now a second similar plane B, also insulated, to be placed parallel to A, and within its compensating atmosphere; it will in part assume the compensation of the plus charge, becoming in consequence itself positively electrical by the retention of its own equivalent of electricity, and thus be in its turn compensated by a stratum of atmosphere, whose electrical condition also will for a similar cause be positive.

58. Under these circumstances the intensity of electrical attraction in B as measured by an electrometer will be greater as its distance from A is less, and less at any particular distance as its atmospheric or other compensation is more perfect.

59. But the compensation and the distance being given, the intensity induced in B will vary as the square of the quantities of free electricity in A directly (8.).

60. Let P and N, fig. 2, be insulated conductors charged to an equal extent, the first positively and the other negatively; each being compensated by the surrounding portions of atmosphere $n n'$ and $p p'$. Let C, a cylindrical conductor,

Fig. 2.



be so interposed midway between P and N, that one of its ends shall by becoming negative assume part of the compensation of P; and its other end by becoming positive assume a corresponding part of the compensation of N. Thus situated, if the force of major attraction be great enough to overcome the minor attraction, all that portion of free electricity in P which is not compensated by the inner spherical stratum of air n' (at a less distance) will pass into the near end of the conductor; at the same time that the plus charge of its other end will pass into N, leaving the latter still negative to the amount of its plus compensation by the inner stratum of air p' . In either of the electrified bodies a residuum

of its former charge will then remain, until by their increased contiguity to the conductor C it is enabled again to assume the compensation.

61. Again, let P N be charged as before, and the conductor C, now terminated by *points*, be introduced as shown in the figure; the free electricity of its plus extremity *p'* will be *retarded* with less force than in the previous case, when its ends were blunt; and the minus common matter of its point *n'* will, in consequence of its reduced size, sustain the compensation of a much larger comparative quantity of free electricity in P, and accordingly exert a corresponding intensity of major force.

62. Such an insulated conductor of given figure as Coulomb has called a *proof plane* applied successively to different parts of the surface of a plus conductor of irregular figure, will during each contact sustain an amount of atmospheric compensation proportionate to that of the part against which it is applied; and hence become charged with a quantity of free electricity proportional to the quantity accumulated on that part. But on removal from the charged surface it will in all the cases acquire the same amount of atmospheric compensation; and consequently in each case an intensity of charge varying as the square of its quantity of free electricity.

63. The preceding deductions of the theory embrace all the most important cases of what is ordinarily called electrical action, that is to say, of the action of free electricity accumulated on the surface of conductors. Their experimental illustrations are too obvious to need, and in so short a paper too multitudinous to admit of, being specified. The accordance of theory and fact may not in all cases appear at first sight, especially if any of the steps in the argument brought forward have been treated in a cursory manner; but if they be attentively considered in series, they will, I apprehend, bear the strictest criticism.

64. In concluding this part of the theory it becomes necessary to add an important observation or two. Without having entered into the question of the *cause* of Coulomb's law, we have hitherto confined ourselves to the facts embraced by it in order to show their perfect consistency with the doctrine of compensation, which we have shown to be a function of the major electrical force and necessarily proceeding from its *definite* nature. I cannot but believe that in doing this I have succeeded in proving that the repulsive force supposed to be inherent in electricity, or in electrified bodies, is superfluous; and that all the phenomena which are commonly ascribed to such

an influence admit of an easy solution on the sole principle of attraction. On this part of the subject there is, however, one seeming difficulty, which may have suggested itself, in as much as it may be thought to be an inevitable consequence of the minor electrical attraction that the electrical atoms be brought together by it into indissoluble union. In reply, I am prepared to show, that neither such an effect, nor any other which is inconsistent with facts, would follow, if we were entirely to expunge *repulsion*, as a principle of action, from our systems of physics.

65. But what I have more particularly to state here is, that it will be seen in the pages which are immediately to follow, that although the law of Coulomb accurately expresses the *sensible effects* of the action of electricity on common matter in general, we have not on record a single instance of that action that may not be minutely and circumstantially traced to matter at *insensible distances**. In the same place we shall be able to explain also the particular action by which the electrical state of one body may become compensated by the opposite electrical state induced in some other body at a sensible distance, and which was purposely passed over in an early article as being premature (7.).

London, April 17, 1838.

X. *On some of the Phenomena and Laws of Action of Voltaic Electricity, and on the Construction of Voltaic Batteries, &c.* By CHRISTOPHER BINKS. *A second Communication, addressed to J. F. Daniell, Esq. F.R.S., &c. Professor of Chemistry in King's College, London. Part the First.*†

Section I.—*Subjects of Inquiry.*

MY DEAR SIR,

Edinburgh, April 9th, 1838.

1. **T**HE paper which I now have the honour to submit to your attention is occupied with the details of an experimental inquiry into subjects which have had their origin as follows:

2. You will remember that in my last paper‡ I stated as the results of certain experiments that any voltaic arrange-

* Since these pages have been in the hands of the printer, I have been favoured by a sight of the forthcoming [Eleventh] series of Dr. Faraday's admirable "Researches;" in which that assiduous and successful philosopher labours to prove by experiment that electrical induction is transmitted to distant bodies by intervening matter. How well this experience accords with the new theory, will more fully appear in the ensuing part of this paper.

† Communicated by Professor Daniell.

‡ Lond. and Edinb. Phil. Mag., July 1837.

ment, whether simple or compound, whose elements were zinc, copper, and dilute sulphuric acid, appeared to be placed in the best circumstances for the exercise of its full power when under either of these two conditions;—first, when the extent of the copper surface was sixteen times greater than that of the zinc; or, secondly, when the surface of the zinc was made the greater of the two in the proportion of about seven to one of copper.

3. To state these results more definitely, let the zinc plate first used have an area, on each surface, of one square inch, and let its associated copper plate be of the same size. The voltaic action resulting from this arrangement being ascertained and taken as unity, and as a standard of comparison, let the zinc and all other conditions remain as at first, but let the copper plate be displaced by others, successively, whose areas increase in a regular progression; when it will be found that with a copper plate of two square inches on each surface, or twice the size of the first, the action will be equal to 1·3; with one of four square inches, equal to 1·6; and so on, by a certain progressive rate of increase till we reach to an area of 16 square inches, when the action will be found to have arrived at its maximum, and to be, in numbers, equal to 4·6.

4. Beyond this point the action will be augmented by no further addition whatever to the size of the copper plate; but, on the contrary, such further additions cause as remarkable a progressive decrease.

5. And, on the other hand, when we retain the copper plate of one square inch first used, but substitute for the small zinc one others of zinc in succession, progressively larger, a corresponding progressive increase of action will likewise follow till the zinc plate becomes of an area of about 7 square inches, at which point the greatest amount of voltaic action will be obtained, being in numbers equal to about 3 compared with the standard amount of 1; and this amount will suffer neither increase nor diminution by any subsequent addition whatever to the dimensions of the zinc plate.

6. The relative proportions of the two plates at which the maximum effect took place as thus determined, were obtained when both surfaces of the zinc, as well as both surfaces of the copper plate, were exposed to the action of the exciting acid; thus making, in the case in which the copper is the larger, a total area of 2 square inches of the zinc and 32 square inches of the copper, and constituting a ratio of 1 to 16.

7. But subsequent experiment (see section 8th) has determined that this same extent of copper surface is needed as well when only one, as when both surfaces of the zinc are

engaged in the operation; provided that if one surface only of zinc be so employed, it be that surface which is directly opposite to the copper. It has been determined that the contrary surface of the zinc serves merely to increase the intensity of action, that is, the quantity in a given time, and in no respect to influence the required area of the copper plate, that being determined solely by the area of that surface of the zinc plate which is opposite to it. These phenomena are more fully entered upon in a subsequent stage of the experiments in this paper; but it is proper to remark here, that the relative proportions of the two plates may be determined under either of the above conditions, either when *both* surfaces of the generating plate or when only *one* is employed; establishing in the former case a ratio of 1 to 16, and in the latter of 1 to 32; but as the latter is the ultimate condition of the experiment, it is that which, in a theoretical point of view, will be considered the more important.

8. Besides some important theoretical considerations which attach to these results, they were immediately serviceable in reconciling the conflicting statements which had previously prevailed respecting the best relative proportions of the two metals; and in showing how, at the will of the inventor, any voltaic battery could be brought to exercise a maximum effect, though different in degree, by having either the zinc or copper plates the larger of the two throughout the series; and also (wherever the same elements are employed) in showing, numerically, the comparative amount of action which can be obtained under any conditions whatever of the proportions of the two metals, and of the strength of the exciting acid.

9. The correctness of these former experiments, so far as they were then carried, is now abundantly confirmed by other experiments differently conducted; and their results will be found ultimately to be deducible from a general law, which will be endeavoured to be established towards the conclusion of this paper.

10. The particular laws formerly arrived at were obtained equally for compound as for single arrangements, and for acid solutions of every strength, or for every degree of activity of the generating agents. But they were sought for under one condition only as regarded the distance of the elementary plates from one another. It was then distinctly stated*, that to preserve uniformity in this respect, the plates were maintained exactly one inch apart throughout the whole inquiry; so that any modification of the results of experiments that

* Page 71, *Phil. Mag.*, July, 1837.

might have occurred by having the mass of interposed fluid of variable dimensions was thus avoided. In only one instance was this deviated from, when the plates were removed from the distance of one inch to that of two inches, which alteration, as will presently appear, was too small materially to affect the results of the experiments as they were then conducted.

11. My first object then is to examine these phænomena (already determined for one such case) when in connexion with every possible condition as regards the distance of the elementary plates from one another; or as they are modified by having the mass of interposed fluid of variable dimensions.

12. Such an inquiry necessarily includes a repetition of my former experiments, since it becomes requisite to find the area of that plate which at any given distance yields the maximum effect, as well as to find the effects abstractedly of differences in the distance, and the influence of acid solutions of different degrees of strength.

13. Until recently, the magnetic needle has usually been employed to detect and estimate comparative quantities of voltaic electricity evolved by ordinary arrangements. When the results brought out by the preceding inquiry had been satisfactorily ascertained, it was made apparent that between them and others previously determined and admitted as correct, there existed a singular disagreement. It became desirable, therefore, to endeavour to reconcile the two methods of observation; and with this view the experiments which occupy the second part of this paper were undertaken; having for their object to determine the relation between the deflections of the magnetic needle and the quantity of zinc and other elements expended in producing those deflections; or, in other words (if the principle of observation here employed be correct), to determine the relation between the quantity of electricity and the deflections it produces.

14. Chemistry has determined the kind of changes which occur among the elements here employed to evolve voltaic electricity; but we know not whether its development be due to the influence of one or more, or all these changes. We have first the resolution of the water into its constituent parts; the appropriation of the oxygen by the zinc, and the appearance of the hydrogen, as gas, upon the copper; the formation of the oxide of zinc, and, subsequently, of the sulphate, and the solution of that salt in the water. The present doctrines of chemistry require that these changes should follow consecutively, but, as far as we can perceive, they are simultaneous.

15. Let some of the attendant phænomena be examined on

the hypothesis, that the development of the electricity, in the present case, is due to the occurrence of a physical change in the condition of the water itself; water is a compound of two measures of hydrogen and one of oxygen. The oxygen being distributed over the whole surface of the zinc, and there combining, it might be presumed that the hydrogen would appear on an extent of surface corresponding to its combining volume, or on a surface of twice the area of that occupied by the other. Or the presumption might be, that the surfaces required would correspond to the difference between the bulks of the metallic zinc expended, and of the liberated hydrogen; or between the bulks of the oxide of zinc produced and of the hydrogen; which latter differences are immense, though not beyond the reach of experiment.

16. But an appeal to experiment at once decides the question, and shows that the relation is none of those just presumed, nor of any other that could have been determined by any *à priori* reasoning. It is found that the relation between the two surfaces upon which the constituents of the water respectively operate or appear, is as one to thirty-two; or directly as the volumes of one quantity by weight of oxygen, and two quantities by weight of hydrogen.

17. So soon as this unexpected relation had been found by actual experiment, it became a matter of interest to inquire whether or not it were an instance of the operation of some general law hitherto unknown, or a mere accidental coincidence. It would have been premature to anticipate the nature of that law on the indications of one instance merely; and it has consequently become the object of the experiments contained in the third part of this paper to examine into as many instances of voltaic action, in connection with the physical and chemical characters of the products resulting from that action, as may be necessary to determine the nature and extent of that law thus suspected to exist.

18. And respecting this part of the present inquiry I may be permitted to add, that whether it be considered with regard to its immediate object, or to the novelty and range of the experiments to which it necessarily leads, to the number of curious relations it serves to detect, or to its bearing upon some others of the collateral sciences, particularly upon chemistry, I know of few others connected with electricity in which an experimentalist can feel a greater interest.

19. The following experiments were begun in the pursuit of these three general objects; but it is almost needless to remark that others have arisen as the inquiry has proceeded, but which will best appear in the order of their occurrence.

The arrangement of the experiments which is here adopted is very nearly the same in the order of succession as that in which they were made, and differs from that order only so far as has appeared desirable, in order the more clearly to show the results actually arrived at, and at the same time to indicate progressively the evidence for other results anticipated, but the completion of which can be reached only at a more advanced stage, or at the conclusion of the whole inquiry. You will permit me also to refer to the circumstance, that occasionally throughout the statements which follow I have thought it desirable to make explicit and repeated references to many minute and well-known details involved in researches of this nature, which in writing to one with whom the subject is so familiar, and to whom each particular would be suggested by the matter in immediate connexion, may appear to be somewhat unnecessary. But as I am aware that my paper may not be confined to your own perusal, I have preferred to aid any demonstrations it may attempt by a recurrence to such details, made explicitly and wherever they might appear useful, rather than, for the sake of inserting only that which is perfectly novel, to omit them, and thus lose the advantage of as much clearness in description as the nature of the subject itself might otherwise admit of.

Section II.—*The Principle of Investigation.*

20. The principle employed throughout the following investigation to detect and estimate effects, has arisen out of the discovery of Faraday of the definite character of voltaic action.

21. The experiments which served chiefly, in the hands of its discoverer, to establish this great principle, were those in which a current of electricity, evolved by a compound arrangement, was passed through water, when it was found, first, that the water which such a current decomposed, bore, in its quantity, a definite relation to the quantity of the elements by whose mutual action that current was produced; and, secondly, that the quantity of any other body than water, which the same current decomposed, was likewise definite, and bore a fixed relation both to the quantity of water first decomposed and to the quantity of elements expended in any one cell of the generating battery itself. And the relations thus presented were found to be exactly the same as the relations between the different respective chemical equivalent numbers of the bodies engaged in the operations. So that, for example, in the instances of the zinc expended in any one cell of the bat-

tery, and of the water or of the muriatic acid which such battery decomposed, the quantities were, for the first, 32, for the second, 9, and for the last 37, or exactly as their equivalent numbers.

22. The distinguished author of this discovery subsequently extended this principle into an inquiry as to the origin and nature of this voltaic action—into an estimate of the absolute quantity of electricity associated with the particles of matter, and also into an estimate of the comparative quantities of electricity evolved by different agencies, &c.; but the general results which I have more particularly in view at this moment are those from which it was deduced that chemical and electrical action, if not identical, are co-existent, and equal in quantity and effect.

23. The application which I make of this principle to the present investigation is exceedingly simple and obvious in its nature: the elements engaged in the phenomena now examined are, water, sulphuric acid, zinc, and copper. Since the last remains unchanged, or subserves its purpose best when unchanged, it may be considered as a mere instrument engaged in aiding the chemical and physical changes which take place among the others, through whose mutual action the electricity itself seems to be evolved. To ascertain the quantity of electricity so evolved, it is only necessary to ascertain (on the principle just stated) the quantity of matter employed in its production; and this can be done by finding the quantity of zinc expended or of sulphuric acid, or of water decomposed, or the quantity of hydrogen evolved, or of sulphate of zinc finally produced. But as chemistry has already determined the relations of these substances one to another, it is only necessary to find the quantity of any one of them, to know the quantity of every other. In the present instance it is most expedient to find the quantity of the zinc so expended by weighing the zinc plate both before and after the operation, or to measure the quantity of the evolved hydrogen; or, if it be the effect of the power of a compound battery which is to be found, then, to measure the quantity of both the gases produced by its action, a contrivance adapted to the last purpose constituting one of the forms of the voltameter of Faraday.

24. The phenomena examined into in the first part of the present paper, are those resulting solely from the action of *single* voltaic arrangements. Let such an elementary arrangement, having its plates of a certain size, and placed at a certain relative distance, be acted on by an acid mixture of a certain degree of strength. The amount of action which will

take place in such an arrangement, under such circumstances, will be altered if the plates be made larger or smaller, or if the strength of the acid or the distance of the plates from one another be altered. But the action resulting from any of these three modifying causes is still of the same kind though different in degree; and it is with the finding of the amount of these differences in degree, in connexion with the circumstances which cause them, that the first part of this paper is chiefly occupied.

25. These degrees of action may either be considered as degrees of chemical or of electrical action. The operations themselves are chemical; and the quantity of chemical action is determined by the quantity of matter which has been employed in it. But this chemical action is induced by a voltaic arrangement; and on the principle stated above, that if not identical, these two kinds of action are co-existent and equal in quantity and effect, the quantities now determined by experiment may be considered as quantities either of chemical or of voltaic action: by whichever name they may be called, the experimental results themselves remain unaffected.

26. These explanations are called for to show the connexion which the method I here employ has with the principle discovered by Faraday; and to show also in what respects its results may be wholly dis-associated from any of the mere theoretical considerations which have followed that discovery. The details which follow are confined solely to the progress and results of experiment; or to such general conclusions only as seem to be warranted by a sufficiency of incontestable evidence.

27. The preceding remarks apply equally to the method used in the 2nd and 3rd parts of the following paper, though the ultimate object in each is different.

28. The particular objects of the 1st and 2nd parts have perhaps been already sufficiently adverted to, but this seems to be the proper place, immediately after the preceding remarks, to define the precise object more particularly aimed at in the third.

29. The researches of Faraday have proved that in one great class of its phenomena the action of electricity is definite; and in another class (more particularly examined in the 3rd part of this paper), but entirely different from any examined by Faraday, it will subsequently be shown that the same principle prevails. In the former case the phenomena so examined related chiefly to the influence of quantity of electricity both as regarded its production and its effects. In the latter case the examinations do not regard the production

or effects of electricity abstractedly, but relate chiefly to some of the physical conditions under which its operations take place, and more particularly to the relative spaces occupied by the agents engaged in its operations. The law deduced by Faraday was, that the influence of electricity over bodies subjected to its action was directly as its quantity, and as the equivalent numbers of the bodies themselves. The results which will be attempted here to be established (but of which one instance only has as yet been adduced, in paragraph 16) are, first, that the superficial areas of the spaces within which this electrical action takes place, have a definite relation to the kind of bodies occupied with that action; or, more specifically, that the superficial areas of the electrodes have a definite relation to the kinds of bodies which, by the force of voltaic action, are determined to those electrodes; and second, that the law which expresses these relations is, that the relation between the areas of the two electrodes is inversely as the relation between the specific gravities of the bodies respectively determined to those electrodes, each area being multiplied by the comparative number of volumes of the body determined to it.

30. So that in the instance already referred to (16), wherein the bodies so determined are hydrogen and oxygen gases, we have their relative specific gravities as 1 and 16; but these bodies have resulted from the binary compound water, in which their volumes are not equal, but as two to one. So that when the numbers expressing the specific gravities are used inversely to represent the areas of the two electrodes, we have those areas as 16 and 1; that is, the area of that electrode at which the hydrogen appears being inversely as the sp. gr. of the hydrogen, is equal to 16, and that of the oxygen equal to 1; which numbers, being multiplied respectively by the number of volumes in which these two bodies occur in this case, give $16 (\text{area}) \times 2 (\text{vol.}) = 32$ for the area of the hydrogen electrode; and $1 (\text{area}) \times 1 (\text{vol.}) = 1$ for that of the oxygen; which proportions are exactly those of the copper and zinc plates as found by actual experiment, the surface of the copper plates yielding the maximum effect in any such arrangement requiring to be 32 times greater than that of the zinc.

31. Or, in another view, the expression of the same law may be, that the relative areas of the two electrodes are directly as the relative bulks of equal weights, multiplied by the number of volumes of the two bodies respectively determined to those electrodes: so that in the same instance as above, in which the elements determined to each electrode are hydrogen and oxygen gases, we have the comparative bulks of equal

weights of each as 16 to 1; but there are two volumes of hydrogen and one of oxygen, so that the bulk of the equal weight of hydrogen, being multiplied by its number of combining volumes, gives $16 \times 2 = 32$, and the oxygen gives $1 \times 1 = 1$, which is precisely the relation between the areas of the two electrodes which is found to obtain when the question is submitted to experiment.

32. And it also follows, that if these relations have been determined for the superficies of any such arrangement, they have been determined likewise for every other geometrical relation peculiar to it; and again, that if they have been ascertained for the electrodes, and the bodies determined to the electrodes, they have also been determined for the compound from which these bodies have been derived, or, in other words, for the electrolyte—so that the expressions of the law just stated may be varied so as to include any or all of these relations.

33. The expressions of this law as just stated are framed in accordance with the present theoretical notions of equivalent numbers, volumes, &c., but it is easy to foresee how investigations of the kind now spoken of, when they shall come to be conducted with sufficient nicety, may be used either to confirm or modify the theories at present entertained on some of those points.

34. This general law will not include all the phænomena attendant upon the operations now referred to; but a series of subordinate laws will be needed to express what have already been distinguished as primary and secondary effects:—but not further to anticipate these results, the preceding remarks will perhaps sufficiently indicate the precise nature of the subject with which the third part of this paper is intended to be occupied.

35. It appears, therefore, that the definite character of voltaic action may be proved to extend into other classes of its phænomena, besides that in which its discoverer first detected it; and it seems unquestionable that the same principle prevails throughout all its operations, and that in the end every class of its phænomena will be found to be governed by a law peculiar to itself. The general law of Faraday, and that just stated, are in perfect harmony with the previously well-known physical and chemical properties of the bodies engaged in the phænomena from which they have been deduced; and these two laws themselves will undoubtedly be found in the end equally to harmonize with each other. It seems not to be an unwarranted probability that if the laws peculiar to every such class of voltaic action were accurately determined, we should

then have furnished to us a collection of data more valuable in themselves, and better calculated perhaps than any yet obtained to enable us to approach to the discovery, if not actually to reveal the real nature of this peculiar but still mysterious agent.

Section III.—*The Method of Investigation and preliminary Experiments.*

36. After some preliminary trials the following method was selected for conducting these experiments. A wooden trough, made water-tight by cement, and measuring 50 inches long, 7 wide, and 7 deep, had its upper horizontal edge marked off from one end to the other into divisions of inches and fourths of inches. At the end from which this graduation began, was fixed the zinc-plate to be experimented with, and the first division, marked 1, was exactly one inch from the surface of the plate itself, and the plate was about one inch from the extreme end of the trough, which consequently was divided, beginning from the zinc-plate, into 48 inches and fourths of inches, or in all into 192 parts. At the zinc end was fixed the cup, holding mercury, in which the connexion between the two experimental plates was to be completed. The zinc plate was connected with a short wire, always of the same kind and length, and each copper plate was soldered to a wire 5 feet in length; so that at whatever position the copper plate might be placed within the trough, whether at the distance of $\frac{1}{4}$ th of an inch from the zinc, or at 48 inches, or at any position intermediate, the wire completing the circuit was always of the same length; these experimental plates were retained at any required position simply by bending their connecting wires twice or thrice at right angles and thus fixing them over the sides of the trough. It being intended to weigh the zinc-plate before and after each experiment, or to collect the gas evolved from the copper plate during the experiment, the former had its connecting wire so short as not to interfere with its being readily weighed and replaced; and the gas from the latter was collected in a funnel-shaped meter having its open base so large as to gather the whole of the gas sent off from the copper plate of whatever size that might be. The long tubular part of this meter was divided into tenths and fiftieths of a cubic inch, and its entire capacity was about $1\frac{1}{2}$ of a cubic inch. The simple contrivance of a couple of strong glass rods, bent twice at right angles, supported by the sides of the trough and stretching across it, served as supporters for the meter, moveable at pleasure, and by which it could be suspended at any required position above the copper plate. The meter was

readily refilled with liquid by immersion in the trough itself. Another trough and meter of larger dimensions were also provided for such experiments as required the use of larger plates than such as could be introduced into the smaller trough.

37. The zinc used throughout the experiments was always of the same kind and always amalgamated. Its equivalent number was 34.5, requiring in consequence of impurities contained in it 34.5 grains, instead of 32, to yield 1 grain of hydrogen. The connecting wires both of the zinc and copper plates were partly covered, but to the same extent in each, with bee's wax, so as to protect them from the action of the acid, so far as they were at any time immersed in it, and to confine the voltaic action entirely to the surfaces of the plates which were the subject of experiment. The circumstance that the surfaces of the wires, protected by the wax, were of uniform extent in every case, will need afterwards to be remembered.

38. These precautions against error, by preserving uniformity in the kind of metal used, in the kind and lengths of the connecting wires, and in the extent of surfaces to be acted upon, are independent of other precautions requiring equally to be observed. A variety of modifying causes are incessantly operating in experiments of this nature, and producing effects of a most perplexing kind, each of which needs to be fully appreciated and guarded against, or as fully as possible corrected, in order to ensure any satisfactory degree of accuracy in the results of experiment.

39. I. It has been shown by yourself*, that immediately after the first immersion of a zinc plate under voltaic arrangement, its amount of action is greatly impeded by an accumulation upon its surface of minute air-bubbles, which adhering to it interpose a surface of air between the plate and the existing acid; thus preventing the full voltaic action so long as they continue to be attached to the plate. By my own experiments I found that a zinc plate, after such accumulation had taken place, yielded a certain measure of gas in 240 seconds, but by repeatedly clearing its surface from these bubbles, by agitating it or otherwise, the same measure of gas was produced in two-thirds the time, or in 160 seconds. When, however, the copper plate is at its maximum size in any arrangement little or no such accumulation occurs.

40. II. The gas arising from the copper plate will do so more rapidly if the water be agitated than if it remain tranquil during the action: an arrangement yielded voltaic action

* Phil. Trans. 1836.

equal to one measure of gas in 80 seconds, during the agitation of the water occasioned by refilling the meter; but after it had become again tranquil the same measure was produced in 95 seconds, or in a length of time greater by about one-fifth. It is almost needless to remark that this increased action is due solely to the mechanical effect of the water when in motion displacing the air-bubbles as rapidly as formed, and more rapidly than they would otherwise be displaced by reason merely of their superior levity.

41. III. Another mechanical effect of a similar kind, but operating to a much greater extent than in either of the preceding instances, is induced by the mere position of the plate from which the gas is evolved: if the surface of a copper plate be placed in a horizontal plane, the gas which is generated on its under side will remain continually attached to the plate; and should that be the only surface operating, the voltaic action of such an arrangement would be almost completely impeded; but, on the contrary, if it be placed and used in a vertical plane, then the only causes likely to obstruct the ready dismissal of the gas from the surface are the comparatively minute ones just referred to in II.

42. IV. A copper wire which had been stretched in order to straighten it, previously to its being attached to a plate, was found to give a voltaic effect about one-fifth less than another, in all other respects the similar, but which had not been so extended; but the conducting power of the former was again restored by exposing it to heat. And, again, two wires, in all other respects alike, but which had accidentally been heated in different degrees, gave a marked difference in amount of action; but had their uniformity of conducting power restored upon exposure to an equal temperature.

43. V. The same remarks apply equally to plates as to wires; but there is another cause of variation in the action of plates not previously recognised. In an extensive course of experiments connected with the subject of this paper, in which I had completed about 15 tables of results, each table containing, on an average, about 10 observations, I was surprised, on looking these tables over, to observe that there was presented invariably (under whatever conditions the experiments might have been performed) a remarkably increased amount of action opposite to a number representing one particular copper plate. Since no probable cause could be assigned for this recurrence, except that of some property peculiar to the plate itself, another plate was substituted for it, when it was found that throughout the whole inquiry the plate in question had been giving nearly double its amount of

action. It was now remembered that this particular plate had been cut from a piece of copper which had been employed in some former experiments, during which it had been partly amalgamated, and had had one of its sides covered with bee's wax, to remove which and the mercury previously to adopting it to its present use, it had been thrown on the surface of a bright fire, and afterwards, whilst nearly red hot, had been plunged in a trough containing dilute sulphuric acid. The same plate was the agent in another curious phænomenon, to be mentioned subsequently. (See Section VIII.) But confining myself to my present purpose, and without further alluding to this circumstance, or to the suggestion it affords of a means of increasing the power of voltaic arrangements, by some such treatment of the plates, it is necessary here to remark that so soon as this condition of the plate was detected, both the whole of the plates, and the results of the experiments in which they had been engaged, were dismissed as uncertain, and the whole repeated with the use of new plates prepared with every possible regard to uniformity in their condition in every particular.

44. VI. The copper plate of any voltaic arrangement very speedily has its surface so affected as to be greatly diminished in its amount of action; an effect arising in some case from a partial action upon the copper itself, but caused more frequently by a deposition of matter upon its surface, derived from the solution it is acting in; and consequently differing in kind and degree according to the nature of that solution, and to the intensity of the voltaic action itself. If a copper plate be so used through a period of 30 minutes, and its amount of action be examined during every five minutes of that time, it will be found that this diminishing in effect will proceed much more rapidly during the latter than the former portions of that time, as is shown in the following table:

No. 1.

Periods of time of 5 minutes each.	}	1st.	2nd.	3rd.	4th.	5th.	6th.
Measures of gas in 50ths of a cubic inch yielded in each time.		35.	33.	29.	24.	17.	9.

And when at the end of these periods the surface of the copper was again brightened, its action was restored to the first amount, or to 35 measures in the same time.

45. This table is not unimportant, inasmuch as it might have occurred that the accelerated action which takes place

by continued immersion upon the zinc plate might be compensated for by the retarded action occurring upon its associated copper plate. But the rates at which the one is accelerated and the other diminished are different, as this table serves to show, and consequently any method of correcting such irregularities of action founded upon these opposite properties of the two plates would be futile.

46. VII. The kind of polish also of the copper plate, and the cleanliness of its surface, have likewise a material influence upon its action, and consequently upon the results of experiments conducted with any measure of accuracy. The plate becomes soiled by frequent handling, and particles of the wax from the adjacent wires are liable to be transferred to its surface, and in the end, by their accumulation, almost totally to obstruct its further action.

47. VIII. Again, the action of any voltaic arrangement is greatly affected by the condition of the surface of the zinc plate, as regards its roughness or smoothness. An irregular rough surface of zinc will give a greater amount of action in a given time than one which has a fine polish.

48. IX. And, on the other hand, an exceedingly irregular or rough surface of zinc, over which a profusion of mercury has been spread, will exhibit a less amount of action than another surface which is perfectly smooth, and over which only so much mercury has adhered as has been required to produce a perfect amalgam. These apparently contrary effects under like circumstances are easily explained:—the plate of zinc which is rough and full of cavities, on being suffused with mercury, presents but here and there a point of zinc amalgam to the action of the acid, the other parts of its surface being pure mercury; whereas, in the other case, in which the surface is smoother and its cavities consequently of less extent, a greater number of amalgamated zinc points are exposed to the action of the acid, and the effect of the plate is consequently greater in proportion to the evenness of its surface.

49. These different conditions of the zinc surface, however, are very different in their effects if the zinc be not amalgamated; for in that case an entirely different principle of action is introduced; but as it is amalgamated zinc alone which is used in these experiments, this particular need not be further alluded to.

50. X. The insoluble impurities contained in common zinc accumulate upon the surface of a plate which has been long used, and unless removed from time to time will also serve in the end, and in addition to those causes already mentioned,

to influence the action of such an arrangement. The consequences of this accumulation become strikingly obvious after a plate has been for some hours in use, and without having had its surface cleansed in the interval.

51. XI. The particular position of the zinc plate whilst in action is needed as equally to be attended to as the position of the copper-plate (III.), though the necessity for this has a very different origin in the two cases. A zinc plate whose surfaces are placed in an horizontal plane will have the dissolved sulphate of zinc resting upon its upper side, and hence interposing a stratum of comparatively inactive matter in the place of the existing acid. But when the plate is used in a vertical position, the dissolved sulphate continually subsides, by its superior gravity, from the surface of the plate; thus occasioning a perpetually renewed contact to take place between it and the fresh exciting acid. This effect may be rendered visible when a glass vessel, holding an arrangement so suspended, is interposed between the eye and a strong light, when the varying refractions caused by the commingling of the two fluids of different densities, viz. the dissolved sulphate and the acid mixture, will show that a stream of the dissolved salt falls continuously from the bottom edge of the plate, and is finally diffused through the mass of surrounding liquid.

52. XII. But the continually accelerating action occurring upon the zinc during immersion is the cause of a more frequent and variable kind of interference in these experiments than almost any other. A zinc plate whose surface is perfectly smooth and truly amalgamated (such as are invariably employed in the subsequent experiments), will give a greater amount of action during a second time of immersion than during the first, as appears by the following table :

No. 2.

Equal times of immersion of 10 minutes each . . .	}	1st.	2nd.	3rd.	4th.	5th.	6th.
Measures of gas in 50ths of a cubic inch yielded in each time		34.	36.	38.	42.	50.	59.

Whatever may be the strength of the acid used, or, in other words, whatever may be the total length of time needed to produce the same measure of gas, yet, when the plate has been affected to the same extent, whether that may have taken place in a longer or a shorter time, the difference between the rates of action in the first and last portions of the total time will be the same as exhibited by this table.

53. XIII. Again, the acid solution becomes weakened by

continued use, and deteriorated by the sulphate of zinc dissolved in it; so that the results of any set of comparative experiments will be untrue, unless this source of variation and error shall have been completely avoided by the constant renewal of the acid, or completely provided against by some system of correction adapted to its varied effects.

54. XIV. A voltaic arrangement, at the commencement of a set of experiments, when the apartment in which they were conducted was comparatively of a low temperature, viz. at 53° Fahr., gave $\frac{1}{10}$ th of a cubic inch of hydrogen in 45 seconds. But on the room becoming warmer, by means of the fire and lights used in it, the action of the arrangement was as progressively increased, and when arrived at the temperature of 60° the same measure of gas was produced in 30 seconds. Attempts were made to alter this activity of the arrangement by altering the surface of the zinc, by re-amalgamating it and immersing the plates in cold water, but without effect; and the original amount of action was restored only after a free admission into the room of a current of a colder atmosphere, during a few hours, had reduced it to the original temperature. It is worthy of remark here, that whilst the atmosphere in the apartment suffered a change equal to 7 degrees of temperature, the liquid the plates were acting in did not alter by more than two degrees; and I have since had occasion constantly to observe that the changes in question, induced by temperature, in the activity of the arrangements, may be brought about equally when the surrounding atmosphere alone (that is, independently of any alteration in the temperature of the plates or the liquid) is the body which has undergone any perceptible change.

55. These effects of the influence of atmospheric temperature, though ascertained by a different method and independently, are in exact accordance, in general character, with those detailed by yourself, and respecting which you remark that "it is now, however, apparent that in the exact measures of different effects which an invariable current of electricity will enable experimentalists to undertake, the variations of atmospheric temperature even must not be neglected*."

56. XV. There is yet another influence affecting the action of voltaic arrangements which has not, that I am aware of, been previously recognised; namely, the mechanical effects of the pressure of the column of liquid resting upon the plates. If in a trough, holding dilute acid and about 12 inches deep, a small zinc plate be fixed at half its depth or at 6 inches, and the associated copper plate of the same size be fixed, first

at the depth of one inch from the surface of the liquid, and afterwards at the bottom or at the depth of 12 inches, but still at the same lateral distance from the zinc, the quantity of voltaic action obtained at each of these positions will be found widely different, as will be seen by the following table, in which the copper plate is moved through the same vertical plane to the different depths successively :

No. 3.

Depth in inches of the copper plate from the surface of the liquid . . .	1.	2.	4.	6.	8.	10.	12.
Time, in seconds, needed at each depth to yield an equal mea- sure of hydro- gen	132"	135"	142"	143"	147"	154"	159"

57. When the plate is near the surface of the liquid the hydrogen arises from it in a regular stream of exceedingly minute bubbles; but when at a greater depth they are dismissed from the plate irregularly and of a much greater size, having apparently adhered to it longer and with greater tenacity than when nearer the surface. It is easy to conceive that previously to the dismissal of these larger bubbles from the surface of the plate, they will have prevented the direct contact of the acid and the copper by an interposed stratum of air, proportionate to the size of their bases, and hence have impeded the voltaic action itself. That these differences are due solely to the influence of pressure, in the present case, will be more distinctly shown subsequently (section 7th), when the precise relative positions of the two plates in these trials will need to be remembered.

58. We perceive in the foregoing enumeration a variety of minute influences and effects, incessantly obtruding themselves into examinations of the nature of this present inquiry; all of which, with the utmost care, need to be provided against, or as far as possible to be corrected, to ensure any degree of accuracy in the results of experiments.

59. For the sake of a compendious reference to the methods by which these sources of error are obviated, or their effects otherwise guarded against throughout this inquiry, they may be classified as follows :

60. 1st. Circumstances merely mechanical, affecting the

action of any arrangement, as position of the plates, agitation of the liquid, adherence of the gas to the plates, pressure of the column of liquid, &c., including the particulars stated in Nos. I., II., III., XI. and XV. of the above enumeration.

2nd. Circumstances of a mixed chemical and mechanical kind, affecting the surfaces of the copper plate, as its polish, cleanliness, &c., and stated in Nos. VI. and VII.

3rd. Circumstances of the same kind, affecting the surface of the zinc, and included in Nos. VIII. IX. and X.

4th. The temper, or conducting power of the wires and plates, as explained in Nos. IV. and V.

5th. The influence of the general temperature under which the experiments are performed, as in No. XIV.

6th. The effects of the weakening and deterioration of the acid mixture in No. XIII.

7th. The accelerated action occurring upon the zinc in No. XII.

61. To provide against each and all of these interferences, I adopt the following plans throughout the whole inquiry, and which being once distinctly pointed out, need not again be referred to in the course of the subsequent details upon other points.

62. The zinc plate is removed from the trough at the end of each single experiment, and its surface cleansed by a linen cloth. The copper plate is polished with fine glass paper after each single experiment, washed with a solution of caustic potash, and then well rinsed in acidulated water before its return for the next experiment. The observations of the measure of gas generated in any given time, is not taken till the water has again become tranquil after the motion occasioned by replacing the plates and refilling the meter; nor till after the complete suspension of the local action which generally takes place upon the first immersion of the zinc. (See 39.)

The operating plates (unless where it is distinctly stated to the contrary,) have their central points always in one straight line, passing horizontally through the centre of the mass of liquid they are acting in, that in each set of comparative experiments the pressure upon the plates may be maintained uniform, and the plates themselves be preserved in a vertical position. The plates of either kind, which are here employed, are cut from the same sheet of metal, the wires from the same coil; and the whole, after being adjusted, are exposed for some time to a like temperature, so that the temper and conducting power, as far as that depends upon the texture of the metal, may be the same in each. The temperature of the liquid and of the apartment in which the experiments are conducted is

maintained as nearly as possible the same during the performance of any one set of experiments.

63. These regulations sufficiently provide against every contingency that can affect the correctness of the experiments, except those of the gradually diminishing activity of the acid mixture through continued use, and the accelerated action upon the zinc which occurs during its continued immersion.

64. Against the effects of these last, especial provision is made as follows: I prepare ten or a dozen of the kind of zinc plates which are the subject of present experiment, and bring each of those by previous management to yield the same amount of action in a given time. This is accomplished by taking one of them as a standard, and by polishing or roughening as may be needed the surfaces of the others, thus bringing each into precisely the same condition; and a little practice makes this to be done with considerable facility.

65. After any one of such a set has been used twice or thrice, its place is supplied with a fresh plate; and after the whole number may have been so employed, the set is repolished and re-adjusted before their application to any new set of experiments. The extreme nicety which this plan introduces into experiments of this kind will be obvious if it be considered that each plate is employed to produce but at most five-tenths of a cubic inch of hydrogen before it is renewed, and consequently will have expended in that effort but little more than three-tenths of a grain of its entire weight; a quantity much too small to affect the action of the plate in any way likely to interfere with that degree of nicety which these experiments require. And with respect to the effect which so small a quantity of zinc will have upon the acid mixture, either by abstracting its active acid, or by impregnating it with the dissolved sulphate, if it be considered that the quantity of acid mixture here employed is seldom less than eight or ten gallons, and that this too is frequently renewed, it will be obvious that the dissolving in it of even many such minute quantities of zinc will have no effect whatever upon the results of experiment.

66. It will subsequently be seen how essential it is that each and all of the above particulars should be strictly attended to; and preparations being thus made and observed throughout, I know of no other circumstances that can affect the experiments or interfere with their accurate performance; and the whole thus arranged admit of being performed with considerable ease and expedition.

67. It has been thought better to place all these particulars under one view, at an early stage of this paper, as well to

show the precautions which have been observed to ensure accuracy, as to avoid the necessity for a continual reference to such details afterwards.

68. The effects now to be sought for, are estimated generally by the length of time needed to produce a certain measure of gas, or for the expenditure of a certain weight of zinc. But it has been shown in some former experiments that the quantities of zinc expended and of hydrogen produced by it, (under certain conditions of voltaic action,) are not always equivalents of one another. In my former paper* a case of this kind is noticed, in which a small battery when used to decompose water, lost zinc, in quantity one third greater than the equivalent of the hydrogen evolved. In the investigation that now follows no uncertainty will be permitted to remain upon this point; and the labour occasioned by such examinations will be found to have been not altogether without use. (See section 9th.) The measure of gas most commonly used is one tenth of a cubic inch, and the time is taken in seconds. With small plates, this measure will be yielded in a length of time ranging between 30" and 900" according to the strength of the acid or the distance of the plates. The moment at which this measure is completed by the meter, may be determined with certainty within two or three seconds of the real time. A closer approximation than this is seldom attempted in the following experiments, nor is it in any respect necessary. The difference between 30 seconds and 900 seconds is an extreme one, and but seldom occurs: most commonly the difference is much less marked, and in some instances is so minute, that no other method, that I am aware of, than that now proposed is adequate to its detection.

69. The capability of this method to detect minute differences in quantity will be apparent upon a little consideration from what has been already said; but the following comparison between it and that afforded by an ordinary magnetic galvanometer, will serve to mark that capability more clearly.

70. The galvanometer here used, though of the common construction, was an exceedingly good one, by Newman; and such an instrument in all respects as would have been employed in the kind of experiments now made had the indications afforded by the needle been desired.

71. The instance selected is an average one of the effects to be estimated. A small arrangement was used at first with its plates one fourth of an inch apart, and afterwards at the distance of thirty-eight inches. At the first position the one

tenth cubic inch of hydrogen was yielded in 45", at the second position in 210". Testing the same phænomenon by the needle, the first position gave a permanent deflection of 60°, and the second of 45°. By the first method the difference detected was equal to the value of the difference between 45 and 210, or 165. By the second, the difference was equal to that between 60 and 45, or 15. Had the divisions of the needle galvanometer been in seconds, or in thirds, or even in tenths of degrees, its indications would still have been inferior in delicacy to those which the watch and meter thus afforded, with the utmost precision and facility.

72. It will appear subsequently that this superior precision, readiness and certainty, do not constitute the only advantages presented over the magnetic needle, and are not the only reasons why this particular method of experimenting should be preferred.

XI. Intelligence and Miscellaneous Articles.

THE HERSCHEL DINNER.

WE deem it proper to record briefly in this Journal the circumstances attendant on the recent festival in honour of Sir John F. W. Herschel, and in commemoration of his return from Southern Africa, after having executed a minute astronomical survey of the Southern Hemisphere, in accordance with the intention and in furtherance of the design of his illustrious father. A meeting of the leading men of science and officers of various scientific institutions in the metropolis having taken place in the apartments of the Geological Society, at a time when the arrival of Sir J. Herschel in his native land was daily expected, to consider and arrange the best means of giving him that welcome with which every lover of knowledge was eager to greet him, it was resolved that a public dinner should be held on the occasion, to which he should be invited, and a vase of silver, to be purchased by the subscriptions of the friends of science, presented to him. Forty-six Stewards were appointed, including several noblemen distinguished by their patronage of science, or their connexion with scientific institutions; His Royal Highness the Duke of Sussex, K.G., P.R.S., having consented to take the Chair, and R. I. Murchison, Esq., F.R.S., V.P.G.S., was appointed Honorary Secretary. The following is a list of the Stewards:

H. R. H. THE DUKE OF SUSSEX, K.G., P.R.S., in the Chair.

STEWARDS.

His Grace the Duke of Northumberland, K.G., F.R.S.

The Marquess of Lansdowne, K.G., F.R.S.

The Marquess of Northampton, V.P.R.S., F.G.S.

The Earl Fitzwilliam, F.R.S., F.S.A.

The Earl of Burlington, V.P.R.S., Chancellor of the Univ. Lond.

The Bishop of Norwich, P.L.S., F.G.S.

Airy, G. B., F.R.S., Astron. Royal.

Babbage, C., F.R.S., Luc. Prof. Camb.

Baily, F., Treas. R.S., P.R.A.S.

Beaufort, Capt., R.N., F.R.S., F.R.A.S.

Broderip, W. J., F.R.S., F.G.S.
 Brodie, Sir B. Bart., F.R.S., P.R.C.S.
 Brown, R., F.R.S., V.P.I.S.
 Buckland, Prof., D.D., F.R.S., F.G.S.
 Children, J. G., V.P.R.S., F.S.A.
 Christie, S. H., Sec. R.S.
 Colby, Col. R.E., F.R.S., Dir. Trig. Surv.
 Cole, Viscount, M.P., F.R.S., F.G.S.
 Daniell, Prof., F.R.S.
 DeMorgan, A., Prof., Sec. R.A.S.
 Egerton, Sir P., Bart., M.P., F.R.S., F.G.S.
 Faraday, Prof., D.C.L., F.R.S.
 Fitton, W. H., M.D., F.R.S., V.P.G.S.
 Gompertz, B., F.R.S., P.M.S.
 Greenough, G. B., F.R.S., F.G.S.
 Gilbert, Davies, V.P.R.S., F.R.A.S.
 Halford, Sir H., Bart., F.R.S., P.R.C.P.
 Hamilton, W., F.R.S., P.R.Geogr.Soc.
 Holland, H., M.D., F.R.S.

Jones, Rev. R., Prof. King's Coll. Lond.
 Konig, C., K.H., F.R.S., F.L.S.
 Lemon, Sir C., Bart., M.P., F.R.S.
 Lubbock, J. W., F.R.S., F.R.A.S.
 Lyell, C., F.R.S., V.P.G.S.
 MacLeay, W. S., F.L.S.
 Murchison, R. I., F.R.S., V.P.G.S.
 Peacock, Prof., F.R.S., F.R.A.S.
 Powell, Prof., F.R.S.
 Rennie, G., F.R.S., F.G.S.
 Rigaud, Prof., V.P.R.S.
 Roget, P. M., M.D., Sec. R.S., F.R.A.S.
 Sedgwick, Prof., F.R.S., F.G.S.
 Smith, Joseph, F.R.S., F.L.S.
 Smyth, Capt., R.N., K.S.F., For. Sec. R.S.
 Somerville, W., M.D., F.R.S., F.L.S.
 Taylor, John, F.R.S., Treas. G.S.
 Walker, J., F.R.S., P. Civ. Eng.
 Whewell, Rev. W., F.R.S., P.G.S.

The Herschel Dinner took place accordingly on Friday the 15th instant, when upwards of four hundred noblemen and gentlemen were assembled, including a large proportion of the most eminent cultivators of science, in all its departments, and of literature and the fine arts, from all parts of the kingdom. Their distinguished guest was seated on the right of the President, who was supported on his left by the Marquess of Lansdowne and Earl Fitzwilliam. After dinner His Royal Highness addressed the company on the occasion of the festival, and presented Sir John Herschel with the vase, which had been placed on the table by Mr. Murchison, and in which were deposited several sheets of paper containing the autograph signatures of the persons present. Sir John Herschel returned thanks in a most interesting address delivered with deep feeling and replete with appropriate reflections. Speeches were afterwards made by Professor Sedgwick, Sir Thomas M. Brisbane, Admiral C. Adam, the Marquess of Lansdowne, Professor Rigaud, the Earl of Burlington, Sir William R. Hamilton, the Rev. W. Whewell, P.G.S., the Marquess of Northampton, the Earl Fitzwilliam, and R. I. Murchison, Esq., Hon. Sec. The last speaker in returning thanks for the stewards, assigned as the reason why he, a geologist, had taken so active a share in the arrangements for this festival, that the distinguished astronomer, in honour of whom it was held, himself claimed the character of a geologist; and he concluded by saying: "May it ever be the pride of our hearts to repeat, 'I was one of those who welcomed Herschel to his native land.'"

Of the Noblemen included in the list of Stewards His Grace the Duke of Northumberland only was absent, and his absence we regret to learn was occasioned by a severe attack of illness. We have, however, great pleasure in adding, that the cost of reducing Sir John Herschel's observations in the Southern Hemisphere will be defrayed by the princely munificence of this nobleman.

The Vase is a splendid copy of the Warwick Vase, in silver, placed upon a pedestal of black marble, on each side of which will appear an inscription in silver relief. That proposed for the principal place, from the pen of H. Gally Knight, Esq., M.P., is as follows:

"Herschel *juniore* ab Afris reduci
 Cœlis australibus exploratis."

In every point of view, whether as regards the just claims of Sir John Herschel on the regard of every lover of science, or the claims of science itself on the estimation of the public, the Herschel Dinner must be considered as one of the most interesting and successful meetings ever held to promote the triumphs of intellect and social virtue. It is with great satisfaction that we have understood that the rank of a Baronet of the United Kingdom has since been conferred upon the illustrious object of this festival.

ACTION OF LIGHT ON SOLUTION OF CYANOGEN.

MM. Pelouze and Richardson have read a memoir on this subject to the Institute, in which they observe that the knowledge which chemistry possesses on the above-named subject is very incomplete. M. Vauquelin was occupied with it in 1818, and showed, that besides ammonia and a peculiar black substance, there were formed by the action of cyanogen upon the elements of water, three distinct acids, namely, carbonic and hydrocyanic acids, and another which he considered as composed of cyanogen and oxygen. His opinion as to the nature of this last substance was entirely founded on theoretic views, for he had not separated his new acid, nor studied any of its combinations. The experiments of MM. Pelouze and Richardson show that M. Vauquelin was in error in announcing the formation of cyanic acid by the decomposition of cyanogen in water, and the substance which he supposed to be cyanate of ammonia was a mixture of urea and oxalate of ammonia.

An aqueous solution of cyanogen, prepared in the usual manner, was exposed to the action of light, until the odour of cyanogen ceased. The solution had a strong smell of hydrocyanic acid; its colour was slightly yellowish, and it was neutral. In the lower part of it a light, black, flocky substance was separated. It was collected in a filter, and freed by distilled water from foreign soluble matter. After this purification it was but slightly soluble in water and in alcohol, insoluble in æther, but on the contrary dissolved by acetic acid and the caustic alkalis, and with bases it formed true salts.

The authors had not so much of this substance to subject to analysis as they could have wished, but they are of opinion that its true composition is expressed by the formula $Az^8 C^8 H^6 O^{14}$.

Part of the liquor was submitted to ebullition, and the vapour disengaged was passed into lime-water. An abundant precipitate of carbonate of lime was formed, which left no doubt as to the formation of carbonic acid during the decomposition of cyanogen in water. The remainder of the liquor yielded during concentration a very sensible quantity of ammonia and hydrocyanic acid. The dried residue had a distinct yellow tint, and a sharp saline taste. Put into alcohol it was divided into nearly equal portions; the soluble portion possessed all the characters of urea; the residue insoluble in alcohol was oxalate of ammonia.

The authors state that the analysis and minute examination of

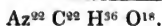
• In this notice the original formulæ are preserved.

these two substances leave no doubt in their mind as to their production during the spontaneous decomposition of cyanogen when dissolved in water. If M. Vauquelin had pursued the examination which he had commenced in the products of this reaction, he would perhaps have first made the admirable discovery, which was effected fifteen years afterward by M. Wöhler, of the artificial production of animal matter.

It is extremely curious to observe a substance of comparatively simple composition, such as cyanogen, give rise to so many different products by its reaction on water.

Admitting $Az^8 C^8 H^8 O^8 [O^1 ?]$, as its composition the decomposition of cyanogen in water may be explained by the following equation :

1	atom of Urea.	$Az^4 C^2 H^8 O^2$.
3	do. Hydrocyanic Acid	$Az^6 C^6 H^6$.
4	do. Carbonic Acid.	$C^4 O^8$.
1	do. Ammonia.	$Az^2 H^6$.
1	do. Oxalate of Ammonia ..	$Az^3 C^2 H^3 O^4$.
1	do. Black Substance.	$Az^8 C^8 H^8 O^4$.



L'Institut, March 1838.

BICHROMATE OF PERCHLORIDE OF CHROMIUM.*

This remarkable compound was discovered by Berzelius ; it was at first called perchloride of chromium, because when put into contact with water it was changed into chromic and hydrochloric acids. Its true composition was ascertained by M. Heinrich Rose.

M. P. Walter gives the following process for preparing this compound : put into a tubulated glass retort an intimate and finely powdered mixture of 100 parts of fused common salt, and 168 parts of neutral chromate of potash ; an S tube is to be put into the tubulure of the retort, through which there are gradually poured 300 parts of concentrated sulphuric acid. The action is rapid from the commencement ; intense red vapours, accompanied by much chlorine, are disengaged. The receiver is to be kept cold to condense the vapour. The acid must be gradually added, or otherwise a loss of the red vapours will take place, and besides this the contents of the retort rise and pass into the receiver. As soon as the acid is added, the retort is to be gently heated, and the heat is to be increased until yellow vapours begin to arise ; the operation is then finished. In the receiver there is found a liquid of an intense red colour, and a solid substance, which, according to M. Dumas, is a compound of this substance with chlorine. By decantation they may be separated, and the liquor when rectified, so as not to obtain the whole of it, yields a compound, the boiling point of which is constant.

The liquid thus obtained is of a magnificent blood-red colour ; it is volatile, and yields fumes abundantly ; when put into a quantity of

* See Lond. and Edinb. Phil. Mag., vol. xii. p. 83.

water it falls to the bottom in drops of an oily appearance, and is converted into chromic and hydrochloric acids. Its boiling point is 244° Fahr., and its specific gravity is 1.71; it acts rapidly on mercury; it is decomposed by sulphur, detonates with phosphorus, dissolves chlorine and iodine, and combines with ammonia with the disengagement of light. A small quantity mixed with concentrated alcohol combines with it with violent explosion, and the inflamed alcohol is projected with force. This unexpected action had nearly deprived M. Walter of his eyesight, and burnt him horribly.

The analysis of this substance by M. Walter agrees with that of M. Rose, namely,

Oxygen	19.28
Chlorine	45.14
Chromium	35.58—100.

Ann. de Chimie. et de Physique, 66.391.

It appears to me that it would be more simple to consider this compound as an oxichloride of chromium, than a bichromate of perchloride of chromium. It might then be regarded as composed of

Two equivs. of Oxygen . . .	16	or	20
One equiv. of Chlorine . .	36	—	45
One equiv. of Chromium . .	28	—	35
	—		—
	80		100

R. P.

METEOROLOGICAL OBSERVATIONS FOR MAY 1838.

Chiswick.—May 1. Fine: rain: fine at night. 2, 3. Very fine. 4. Dry haze. 5. Thunder: fine. 6. Slight haze: very fine. 7. 8. Very fine. 9. Hot and very dry. 10. Cold and dry. 11, 12. Fine. 13. Hazy: fine. 14. Cloudy and cold. 15—18. Fine. 19. Overcast. 20. Slight rain. 21. Cloudy. 22, 23. Cloudy: rain. 24. Overcast. 25—27. Fine. 28. Rain. 29, 30. Fine. 31. Very fine: heavy thunder-showers at night.

Boston.—May 1. Cloudy: heavy rain P.M. 2. Cloudy. 3. Cloudy: thunder, lightning, and rain early A.M. 4, 5. Fine. 6. Cloudy. 7—12. Fine. 13. Cloudy: rain early A.M.: rain P.M. 14. Cloudy: rain early A.M. 15—17. Fine. 18—19. Cloudy. 20. Cloudy: rain P.M. 21. Cloudy. 22. Cloudy: rain A.M. 23. Rain. 24, 25. Cloudy. 26, 27. Fine. 28. Fine: rain P.M. 29. Cloudy. 30. Cloudy: rain P.M. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—May 1. Sun shone: hoar frost A.M.: cold P.M. 2. Sun shone: genial rain afternoon. 3. Sun shone, showery A.M. 4. Sun shone: moist and mild: genial. 5. Sun shone: a beautiful day. 6. Sun shone: very dry and parching. 7. Sun shone: warm and genial. 8. Sun shone: very warm and clear. 9. Sun shone: the same. 10. Easterly wind: cool. 11. Sun shone: milder than preceding day. 12. Sun shone: a few drops of rain P.M. 13. Sun shone: withering day. 14. Sun shone: cold, with hail showers. 15. Sun shone: clear and cold. 16. Sun shone: heavy hail showers. 17. Sun shone: cold and showery. 18. Sun shone: cold and withering. 19. Cold and very wet. 20. Heavy rain all day. 21. Sun shone: showery and mild. 22. Mild and very wet. 23. Sun shone: moist: rather cold. 24. Sun shone: clear growing day. 25. Sun shone: mild and clear. 26. Sun shone: clear and warm. 27. Dull and withering. 28. Sun shone: clear and warmish. 29. Soft air: wet all day. 30. Sun shone: warm and growing. 31. Sun shone: mild with showers.

Days of Month, 1888. May.	Barometer.				Thermometer.				Wind.				Rain.				Dew- point. Lond. Roy. Soc. 9 a.m.			
	Lond. : Roy. Soc. 9 a.m.	Chilswick.		Boston. 8 1/2 a.m.	Dumfries-shire.		Lond. : Roy. Soc. 9 a.m.	Chilswick.		Dumfries-shire. 9 a.m.	Dumfries-shire. 9 p.m.	Lond. : Roy. Soc. 9 a.m.	Chilswick.	Boston.	Dumfries-shire.					
		Max.	Min.		Max.	Min.		Max.	Min.											
1.	29.632	29.699	29.625	29.18	29.45	29.45	43.8	53.0	45.3	68	48	43	42	38	ssE.	calm	NW.	.047	.10	42
2.	29.834	29.828	29.718	29.30	29.54	29.54	59.3	61.3	48.3	68	49	56.5	50	45	S.	W.	SW.	.111	.69	48
3.	29.870	29.986	29.852	29.25	29.47	29.75	56.7	64.6	51.7	67	35	58	52	45	S.	W.	S.SW.21	50
4.	30.014	30.034	29.973	29.41	29.85	29.98	58.4	63.0	48.8	70	48	60	51	43	NE.	calm	SE.	52
5.	30.136	30.213	30.107	29.57	30.13	30.23	58.9	65.6	49.4	67	44	57.5	54	42	NW.	NE.	SE.	50
6.	30.308	30.326	30.288	29.88	30.25	30.23	47.4	64.4	45.8	64	36	46	49	49	NE.	calm	SE.	.105	...	47
7.	30.328	30.348	30.293	29.81	30.24	30.25	54.4	61.4	42.3	76	45	55	59	51	N.	calm	S.	42
8.	30.328	30.317	30.231	29.78	30.25	30.18	64.2	73.2	50.3	78	40	59	62	55	NE.	calm	NW.	51
9.	30.200	30.194	30.142	29.65	30.12	30.11	59.9	73.3	48.8	77	41	61	67	44	N.	calm	N.57	50
10.	30.346	30.416	30.320	29.92	51.3	70.3	43.8	59	30	51	49	45	NE.	calm	N.	42
11.	30.394	30.406	30.255	29.99	30.28	30.13	49.4	55.3	38.3	63	27	52	53	45	NE.	calm	E. & S.	38
12.	30.148	30.175	29.851	29.72	30.00	29.70	53.4	58.4	41.7	71	33	53	55	44	N.	calm	NW.	41
13.	29.686	29.722	29.644	29.24	29.63	29.62	49.7	63.6	46.2	61	30	50	51	36	NW.	calm	N.04	44
14.	29.668	29.709	29.676	29.36	29.60	29.61	46.8	56.2	40.4	52	28	45	43	33	NW.	calm	NNE.23	38
15.	29.764	29.822	29.768	29.44	29.66	29.67	45.2	53.6	36.2	57	26	46	48	41	N.	calm	N.	35
16.	29.858	29.869	29.446	29.45	29.67	29.68	47.4	54.7	36.8	59	28	48	48	38	E.	calm	NW.	32
17.	29.812	29.867	29.806	29.40	29.68	29.77	47.3	52.8	38.9	60	30	48	41	36	NE.	calm	N.	37
18.	29.866	29.983	29.864	29.39	29.75	29.77	49.3	55.4	39.0	54	31	48.5	47	39	NW.	calm	E.	38
19.	29.826	29.951	29.734	29.55	29.72	29.48	50.0	55.2	40.9	62	48	48	48	39	E.	calm	N.	.03	...	40
20.	29.592	29.615	29.507	29.17	29.40	29.35	52.5	58.6	49.6	59	49	55	49	49	E.	calm	E.	.01	...	45
21.	29.540	29.617	29.530	28.93	29.32	29.38	56.7	65.0	50.2	64	50	54.5	49	47	S.	calm	SE.	.038	.06	46
22.	29.530	29.541	29.414	29.05	29.38	29.38	54.5	60.7	52.0	61	45	56	48	47	SE.	calm	NW.	.02	.10	50
23.	29.796	29.832	29.798	29.20	29.57	29.72	55.0	59.3	46.8	48	46	52	50	47	NW.	calm	N.	.033	.02	45
24.	29.900	29.990	29.915	29.50	29.88	29.97	51.7	57.4	48.3	62	46	51	51	46	NW.	calm	E.	.041	.02	46
25.	30.012	30.053	30.007	29.63	29.97	29.98	49.7	57.7	46.8	66	45	49.5	54	48	N.	calm	SE.	47
26.	30.076	30.085	30.053	29.66	30.00	30.04	50.8	61.2	46.2	64	40	55	54	48	NE.	calm	E.	47
27.	29.978	29.989	29.790	29.53	29.96	29.80	51.4	59.8	42.6	64	45	54	54	47	NE.	calm	E.	.05	...	44
28.	29.560	29.589	29.565	29.16	29.67	29.55	53.4	56.7	48.2	60	45	60	57	49	ENE.	calm	SE.	.077	.21	50
29.	29.628	29.697	29.621	29.11	29.44	29.50	60.2	69.7	51.6	68	50	58	49	46	SSW.	calm	SE.	.166	.18	52
30.	29.808	29.881	29.788	29.23	29.60	29.70	59.3	70.4	52.8	72	43	59	55	49	S.	calm	SE.	.013	...	51
31.	29.924	29.932	29.906	29.42	29.75	29.80	62.2	67.0	52.2	75	48	55	56	49	S.	calm	NE.	.03	.03	53
Mean.	29.915	29.957	29.854	29.44	29.77	29.79	53.4	61.3	45.8	64.38	53	55	55	44	Sum.92	1.88	Mean. 44.9

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[THIRD SERIES.]

AUGUST 1838.

XII. *On the Conditions of Equilibrium of a Homogeneous Planet in a Fluid State.* By JAMES IVORY, K.H., F.R.S., &c. &c.*

THIS problem is treated in the *Traité de Mécanique* of Poisson, a work which is in everybody's hands; and, in order to abridge, I shall take the differential equation of a level surface from that work (Equat. (b), p. 536, vol. ii. edit. 2nd,) viz.

$$X dx + Y dy + Z dz + \alpha^2 (x dx + y dy) = 0:$$

here x, y, z are the rectangular coordinates of a point in the level surface, z being parallel to the axis of rotation: X, Y, Z , are the attractions of the whole mass upon the particle, in the respective directions of x, y, z : and α is the angular velocity of rotation about the axis. By integrating the foregoing equation we obtain,

$$\text{Const.} = \int (X dx + Y dy + Z dz) + \frac{\alpha^2}{2} (x^2 + y^2) \dots (1.)$$

The whole mass being divided into two parts by the level surface, I shall put P, Q, R , for the attractive forces parallel to x, y, z , which the matter within the level surface exerts upon the particle in that surface; and P', Q', R' for the like attractions of the matter without the level surface: so that we shall have

$$X = P + P', \quad Y = Q + Q', \quad Z = R + R';$$

and the equation of the level surface will be

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 13. No. 80. Aug. 1838. G

$$\text{Const.} = \int (P dx + Q dy + R dz) + \frac{a^2}{2} (x^2 + y^2) \\ + \int (P' dx + Q' dy + R' dz).$$

Now the least attention to the nature of this equation will show that the attraction of the matter without the level surface is entirely independent of the rest of the equation. As there is no definite relation between P , Q , R and P' , Q' , R' , these functions denoting the attractive forces of different quantities of matter, a level surface will have no determinate figure, unless we divide its equation into two separate parts containing the unrelated quantities, viz.

$$\text{Const.} = \int (P dx + Q dy + R dz) + \frac{a^2}{2} (x^2 + y^2) \dots\dots (2.)$$

$$\text{Const.} = \int (P' dx + Q' dy + R' dz) \dots\dots\dots (3.)$$

Such are the equations of equilibrium of a homogeneous planet supposed fluid: but it will be more convenient to use the two following, of which equation (1.) is the sum of (2.) and (3.),

$$\text{Const.} = \int (X dx + Y dy + Z dz) + \frac{a^2}{2} (x^2 + y^2) \dots (1.)$$

$$\text{Const.} = \int (P' dx + Q' dy + R' dz) \dots\dots\dots (3.)$$

Now these two equations are the same with those given in a paper in the Philosophical Transactions for 1824, and in two subsequent papers written for the purpose of obviating some objections (I had almost said, frivolous objections) of M. Poisson*. The foregoing very simple investigation proves the justness of the solution of the problem in the papers cited; and at the same time shows the insufficiency and incorrectness of making the figure of equilibrium depend on one equation. (Poisson, *Mécanique*, p. 550, vol. ii., edit. 2nd.)

What is said is sufficient for the present purpose: but the subject deserves a more extended discussion; because, by the procedure here followed, the greatest simplicity and clearness are introduced in one of the most perplexed and imperfect theories, that occur in the system of the universe.

July 10, 1838.

JAMES IVORY.

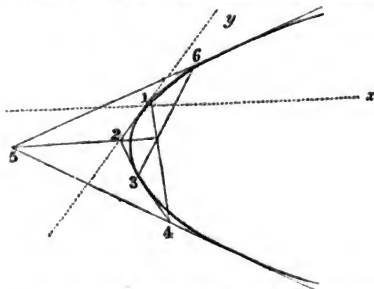
* See Phil. Mag., First Series, vol. lxiii. p. 339; vol. lxv. p. 241; vol. lxvi. p. 429; vol. lxvii. p. 31, 82, 439.—EDIT.

XIII. *On a Property of the Conic Sections.* By J. W. LUBBOCK, Esq.*.

IF any hexagon be circumscribed about any conic section, and the opposite angles be joined, the three diagonals have a common intersection†.

This remarkable theorem was first given by M. Brianchon in the 13th cahier of the *Journal de l'Ecole Polytechnique*, p. 301. It was deduced by M. Brianchon through Pascal's celebrated property of the inscribed hexagon, but it seems desirable to obtain a direct proof of this curious theorem, and in so doing I have found an equation of condition between the coordinates of the angles of the circumscribed hexagon, upon which the property in question may be said to depend.

Let the angles of the circumscribed hexagon be denoted by the figures 1, 2, 3, 4, 5, 6, as in the annexed diagram. The lines 1 4, 3 6 and 2 5 have a common intersection.



Let α, β be the coordinates of the intersection of the line 1 4 with 2 5 then, x_1, y_1 being the coordinate of the point 1, &c.

$$y_1 - \beta = \frac{y_1 - y_4}{x_1 - x_4} (x_1 - \alpha)$$

$$y_2 - \beta = \frac{y_2 - y_5}{x_2 - x_5} (x_2 - \alpha)$$

$$\alpha = \frac{(x_4 y_1 - y_4 x_1)(x_2 - x_5) + (x_5 y_2 - y_5 x_2)(x_1 - x_4)}{(y_1 - y_4)(x_5 - x_2) + (y_2 - y_5)(x_1 - x_4)}$$

and if α, β are also the coordinates of the intersection of the lines 1 4 and 3 6, so that the lines 1 4, 2 5, and 3 6 have a common intersection,

$$\alpha = \frac{(x_4 y_1 - y_4 x_1)(x_3 - x_6) + (x_6 y_3 - y_3 x_6)(x_1 - x_4)}{(y_1 - y_4)(x_3 - x_6) + (y_6 - y_3)(x_1 - x_4)}$$

* Communicated by the Author.

† On this subject see also a paper by Mr. Davies, *Phil. Mag.*, First Series, vol. lxviii. p. 337.—EDIT.

Equating these values of α , we obtain the following equation of condition, upon which the truth of the theorem in question depends,

$$\begin{aligned} & (y_1 - y_4) \{ (x_3 - x_6) (x_5 y_2 - y_5 x_2) + (x_5 - x_2) (x_6 y_3 - y_6 x_3) \} \\ & + (y_2 - y_5) \{ (x_1 - x_4) (x_6 y_3 - y_6 x_3) + (x_6 - x_3) (x_4 y_1 - y_4 x_1) \} \\ & + (y_3 - y_6) \{ (x_2 - x_5) (x_4 y_1 - y_4 x_1) + (x_4 - x_1) (x_5 y_2 - y_5 x_2) \} \\ & = 0. \end{aligned}$$

It remains to prove that this equation does hold good, in consequence of the relations which exist between the quantities contained in it.

We may make $x_1 = 0$, $x_2 = 0$ without limiting the generality of the theorem, the direction of the coordinate axis y being any whatever; this amounts to taking for the origin the point in which the line 1 2 touches the parabola. In this case the points 5 and 4, 6 and 3, 1 and 2 are symmetrical and similarly involved, and the preceding equation of condition becomes

$$\begin{aligned} & (y_1 - y_4) x_5 \{ x_3 (y_2 - y_6) + x_6 (y_3 - y_2) \} \\ & + (y_2 - y_5) x_4 \{ x_6 (y_1 - y_3) + x_3 (y_6 - y_1) \} \\ & + (y_3 - y_6) x_4 x_5 (y_2 - y_1) = 0. \end{aligned} \quad \text{A.}$$

The equation to the tangent of the parabola $y^2 = p x$ passing through the points (z, y) (α, β) is

$$p(x - \alpha)^2 - 4y(x - \alpha)(y - \beta) + 4x(y - \beta)^2 = 0 \quad \text{B.}$$

If x, y coincide with the point 2, so that $x_2 = 0$, and if α, β coincide with the point 3,

$$x_3 = \frac{4y_2(y_3 - y_2)}{p}.$$

Similarly
$$x_6 = \frac{4y_1(y_6 - y_1)}{p}.$$

Again, if in the equation B the point (x, y) coincide with the point 3, the two values of $\frac{x - \alpha}{y - \beta}$ correspond to the directions of the lines 3 2 and 3 4.

$$\frac{x_3 - \alpha}{y_3 - \beta} = \frac{2y_3 \pm 2\sqrt{y_3^2 - px_3}}{p}$$

Making $\alpha = x_2 = 0$, $\beta = y_2$, and taking the upper sign

$$\begin{aligned} x_3 &= \frac{(y_3 - y_2)(2y_3 - 2\sqrt{y_3^2 - 4y_2(y_3 - y_2)})}{p} \\ &= \frac{4y_2(y_3 - y_2)}{p} \text{ as before.} \end{aligned}$$

Taking the lower sign and making α, β coincide with the point 4.

$$x_4 = \frac{4(y_3 - y_2)(y_4 - y_3 + y_2)}{p}.$$

Similarly, because the points (5, 4), (6, 3) and (1, 2) are symmetrical,

$$x_5 = \frac{4(y_6 - y_1)(y_5 - y_6 + y_1)}{p}.$$

Again, by equation B,

$$x - x_4 = \frac{(y - y_4) \{2y_4 \pm 2\sqrt{y_4^2 - p x_4}\}}{p}$$

if the upper sign refer to the direction of the line 4 5, the lower refers to the direction of the line 4 3.

$$x_5 = \frac{4(y_4 - y_3 + y_2)(y_5 - y_4 + y_3 - y_2)}{p}.$$

Equating this value of x_5 to that found above,

$$(y_4 - y_3 + y_2)(y_4 - y_3 + y_2 + y_5) + (y_6 - y_1)(y_5 - y_6 + y_1) - 0$$

$$y_4 - y_3 + y_2 = \frac{y_5 \pm \sqrt{y_5^2 - 4(y_6 - y_1)(y_5 - y_6 + y_1)}}{2}.$$

Hence $y_4 - y_3 + y_2$ is equal to $y_5 - y_6 + y_1$, or to $y_6 - y_1$. Employing the values of x_3, x_4, x_5 and x_6 which have been found, and the remarkable equation of condition

$$y_4 - y_3 + y_2 = y_5 = y_6 + y_1 \text{ or}$$

$$y_1 - y_2 + y_3 - y_4 + y_5 - y_6 = 0$$

it is easy to verify the truth of the equation A, p. 84.

This equation may now be put in the form

$$(y_1 - y_4)(y_6 - y_1)(y_3 - y_2)(y_4 - y_3 + y_2) \{y_2(y_2 - y_6) + y_1(y_6 - y_1)\}$$

$$+ (y_2 - y_3)(y_3 - y_2)(y_6 - y_1)(y_4 - y_3 + y_2) \{y_1(y_1 - y_3) + y_2(y_3 - y_2)\}$$

$$+ (y_3 - y_6)(y_3 - y_2)(y_4 - y_3 + y_2)^2(y_6 - y_1)(y_2 - y_1)$$

$$= (y_6 - y_1)(y_3 - y_2)(y_4 - y_3 + y_2) \left\{ (y_1 - y_4) \{y_2(y_2 - y_6) + y_1(y_6 - y_1)\} \right.$$

$$\left. + (y_2 - y_5) \{y_1(y_1 - y_3) + y_2(y_3 - y_2)\} + (y_3 - y_6)(y_4 - y_3 + y_2)(y_2 - y_1) \right\}$$

putting for $y_2 - y_5$ its value $y_3 - y_4 + y_1 - y_6$, the quantity between the brackets becomes

$$(y_1 - y_4) \{y_2(y_2 - y_6) + y_1(y_6 - y_1)\}$$

$$+ (y_3 - y_4 + y_1 - y_6) \{y_1(y_1 - y_3) + y_2(y_3 - y_2)\}$$

$$+ (y_3 - y_6)(y_4 - y_3 + y_2)(y_2 - y_1)$$

$$= (y_1 - y_4) \{y_2(y_2 - y_6) + y_1(y_6 - y_1) + y_1(y_1 - y_3) + y_2(y_3 - y_2)\}$$

$$\begin{aligned}
 &+ (y_3 - y_6) \{y_1 (y_1 - y_3) + y_2 (y_3 - y_2) + (y_4 - y_3 + y_2) (y_2 - y_1)\} \\
 &= (y_1 - y_4) (y_3 - y_6) (y_2 - y_1) + (y_3 - y_6) (y_1 - y_4) (y_1 - y_2) \\
 &= 0, \text{ which proves the truth of the theorem in question.}
 \end{aligned}$$

If $y_4 - y_3 + y_2 = y_6 - y_1$

$$y_5 - y_6 + y_1 = y_3 - y_2$$

$x_4 = x_5$, $y_4 = y_5$, the points 4 and 5 coincide, and by reference to the figure it will easily be seen that it is useless to coincide this case.

The equation of condition

$$y_1 - y_2 + y_3 - y_4 + y_5 - y_6 = 0$$

has been found upon the supposition, that $x_1 = 0$, $x_2 = 0$, which simplifies the expressions. But it is easy to show by the transformation of coordinates that if the above equation be true, with such limitations, it is also true in the more general case, the only limitation required being, that the axis x be parallel to the axis of the parabola; and then, however the circumscribed hexagon be situated

$$y_1 - y_2 + y_3 - y_4 + y_5 - y_6 = 0.$$

In the *Phil. Mag. and Annals*, N.S., 1829, vol. vi. p. 249, I gave a direct proof from the equation to the parabola $y^2 = px$ of Pascal's celebrated property of the inscribed hexagon, and I showed that the proof might be extended to the general equation of the conic sections $y^2 = p'x + q'x^2$, by substituting for the coordinates x and y of any point

$$\frac{x \frac{p'^2}{p}}{p' + q'x} \text{ and } \frac{\frac{p'y}{p}}{p' + q'x} \text{ respectively.}$$

By such substitutions all the preceding expressions which are true for the parabola $y^2 = px$ may be extended to the conic sections generally, which are included under the equation $y^2 = p'x + q'x^2$. Thus the equation of condition

$$y_1 - y_2 + y_3 - y_4 + y_5 - y_6 = 0 \text{ becomes}$$

$$\frac{y_1}{p' + q'x_1} - \frac{y_2}{p' + q'x_2} + \frac{y_3}{p' + q'x_3} - \frac{y_4}{p' + q'x_4} + \frac{y_5}{p' + q'x_5} - \frac{y_6}{p' + q'x_6} = 0$$

XIV. *Experimental Researches on Combustion and Flame.*

By DAVID WALDIE*.

[Illustrated by Plate I.]

THE subject of combustion has long engaged the attention of the most distinguished chemists, and the results of their inquiries are incorporated more or less in the various

* Communicated by the Author.

treatises on chemistry; an excellent account of them may be found in Ure's Chemical Dictionary, under the article Combustion. The subject however is not exhausted, and there are still some questions which by a new method of investigation may I think now be decided, and several phænomena hitherto unaccounted for which may now be explained.

The principal or essential circumstance in combustion is the fact of combination between two substances of opposite electrical energies. One of these, as being the apparent source of the heat and light, is called the combustible; the other, as absolutely necessary to the phænomena, the supporter of combustion. This has given rise to the division of bodies into these two classes, supposed by some to be remarkably distinguished from each other by their part in the process as they are in their place on the electro-chemical scale. Others again have contended that there is no essential distinction between the two classes of bodies with respect to this phænomenon; that in fact both are equally entitled to the name of combustible, and that the heat and light evolved are simply indications of energetic chemical action between any two substances: in proof of this various instances have been adduced, such as the combustion which takes place between iron or copper filings and sulphur, between potassium and cyanogen or sulphuretted hydrogen, between vapour of anhydrous sulphuric acid and dry baryta, as noticed by Bussy, magnesia and sulphuric acid, &c. in all of which we have all the phænomena of combustion, without the presence of any of those substances exclusively called supporters of combustion, or in some of these cases without either combustibles or supporters.

The latter explanation seems now to be admitted so far by chemists in general, but yet it occurred to me that its truth was susceptible of still better demonstration, and that instead of searching for particular instances of combustion it might be supported by a much more general proposition: that in fact if this were the true explanation, it should follow as a general rule, that if what is commonly called a combustible burn in a supporter, a supporter ought also to burn in a combustible. This accordingly on trial I found to be the case.

The apparatus employed for these experiments consisted of a wide-mouthed flask (about 8 or 9 inches long) having cemented to it a cap of tinned iron, pierced with four holes: to two of these two brass sockets were soldered, made to fit the ends of two flexible tubes proceeding from two gas-holders; to the two other holes were attached small pieces of tube, over one of which a piece of sheet caoutchouc was

tied, through which a slender platinum or iron wire could be passed to try the temperature of different parts of the flame, and to the other was fixed a bladder in order to allow of expansion. The flask was filled by being immersed in a trough of water; the water was then displaced by inserting one of the flexible tubes into one of the sockets, and causing gas to flow into it from a connected gas-holder, the water escaping by the other socket; the tube was then removed, corks inserted in the sockets, and the flask placed on a retort stand, with its mouth downwards and the bladder hanging flaccid: the tube was then replaced in the socket so as to supply more gas if necessary. Now when the flask was filled with one of the common supporters, such as oxygen, and one of the common combustible gases was to be burnt in it, the method requires no explanation. When again the oxygen was to be burnt in hydrogen the cork was removed from the socket, and the gas set fire to, being made to flow gently from the flask, in order to prevent the combustion from getting inwards; the oxygen then being made to flow with a proper degree of force from a small brass jet fixed on the end of another flexible tube, communicating with another gas-holder containing oxygen, was passed steadily through the burning hydrogen into the flask, and the end of the flexible tube pushed home into the socket; the hydrogen burning outside the flask was now extinguished, and the oxygen found burning within.

When again the gas was not confinable in a common gas-holder with water, such as nitrous acid vapour, it was prepared in a wide-mouthed flask; and when this was believed to be full, a jet of hydrogen burning from a brass nozzle fixed to the turned-up extremity of a glass tube connected with a flexible tube was let down into it: or, for instance with chlorine, a jar was filled with this gas over the water-trough, and a jet let down as before, a tin plate being fixed to the tube so as to cover the jar and allow the jet to descend to near the bottom. If again these gases were to be burnt the materials were placed in a small flask, to which was fixed a tube having a brass piece to fit the socket of the flask, and a jet placed on its extremity.

By means of such apparatus oxygen was made to burn in atmospheres of hydrogen, olefiant gas, coal gas, sulphuretted hydrogen and carbonic oxide; nitrous oxide was burnt in hydrogen and coal gas; nitrous acid vapour in hydrogen; chlorine in hydrogen, and mixtures of these with nitrogen or carbonic acid, common air for instance, in the same gases.

By these experiments, oxygen, atmospheric air, nitrous oxide, nitrous acid and chlorine are shown to be not only

really but also apparently as much combustibles as hydrogen or coal gas, and these again are exhibited in the form of supporters of combustion. In fact this distinction arises simply from the accidental circumstance of oxygen being contained in our atmosphere, and of hydro-carbonaceous substances being emitted from other bodies into it: there is really no distinction between the two, the phenomena of combustion proceeding from *the act of combination* of the two oppositely electrical substances.

I did not succeed in making iodine vapour burn in hydrogen, nor the reverse; the affinity for each other seems not sufficiently strong. Nitric oxide does not burn in hydrogen, nor *vice versâ*; a circumstance remarkable enough, as the two constituents of this gas are in respect to their density in precisely similar circumstances to a simple mixture of the two; no more however than might have been expected from the circumstance that a mixture of the two does not detonate but burns with flame in contact with the air. It is said in some works on chemistry, that the products of this combustion are water and pure nitrogen: this I suppose must have been a conclusion made beforehand, on the supposition that the nitric oxide is decomposed completely: it would appear rather that the oxygen for the combustion of the hydrogen is supplied from the air, and that the nitric oxide is either not decomposed, or at least only partially, as I find copious nitrous acid fumes produced, whether the experiment is performed by burning them together in a jar even with excess of hydrogen, or by burning the mixture from a jet either in oxygen, or oxygen in it.

I had made but very few of these experiments until I perceived that there was a great variety in the appearance of the flames. The jet of hydrogen for instance, in passing from air to oxygen was observed to shrink in size, and become brighter and denser; the jet of oxygen in hydrogen was observed also to be much smaller than that of hydrogen in oxygen. The flame of hydrogen in chlorine, nitrous oxide, or nitrous acid was much larger than in air; the flame of these gases in hydrogen again was small and concentrated. In all these cases the gases were made to issue from the same jet, with as nearly as possible equal degrees of velocity.

Before making any observations on the causes of these differences, it will be necessary to take some notice of the structure of flame. This has been studied chiefly in the flame of a common candle or jet of coal gas. It consists then, according to observations already made, of, 1st, a dark central portion consisting of unmixed, unconsumed gas; 2ndly, a dense

very luminous white portion surmounting the dark portion which penetrates further up its centre than its circumference; this consists of solid particles of charcoal burning at a white heat; 3rdly, a blue cap arising from the bottom and surrounding the dark portion, extending moreover some way up the sides of the white part, this being supposed to be composed of the gas burning in an undecomposed state; 4thly, an envelope of a light blue colour, of a pinkish or lilac shade, consisting of the proper combustible mixture, this being the hottest part of the flame: this part is thinnest at the bottom, increasing in thickness to the top where, of course, the heat is most intense; and, 5thly, another envelope of a yellowish brown colour, chiefly covering the upper part, and consisting of unconsumed matter or products of combustion. These two latter are not well seen in the flame of coal gas, on account of the great size and brightness of the luminous white portion.

The explanation of these appearances is obvious enough: the gas issuing from the jet spreads out into the atmosphere, all it mixes with a sufficient quantity of oxygen to form a combustible mixture; as it ascends, however, it becomes strongly heated by the surrounding flame, deposits solid charcoal, the combustion of which forms the white part of the flame.

When a jet of coal gas or olefiant gas burns in oxygen the parts of the flame are all the same, but differing considerably in appearance; the dark portion, the white portion, and the blue cap 1, *b*, *a*, (Plate I.) fig. 1, are greatly diminished in size, looking as it were compressed, the white being however more brilliant. The light blue portion again, 2, is greatly enlarged, the shell of flame becoming much thicker, and the heat is much greater, easily fusing a small platinum wire. The yellow tail, 3, also is now perfectly visible. The whole flame is also much smaller than in air. These changes are due simply to the more perfect combustion produced by the oxygen being undiluted, so that the greater part of the gas at once undergoes perfect combustion; the diminution of size of this as well as of flames in general, in oxygen compared with air, arising from the circumstance, that in the latter the gas requires to penetrate or diffuse itself over a much larger space before it meets with a sufficient quantity of air to produce full combination.

In flame again, where there is no solid matter deposited, as in that of hydrogen, the appearances are much simpler; in this case (burning in oxygen) it consists of a dark central portion of unmixed gas, 1, of a light lilac blue envelope of ex-

plosive mixture, 2, and of the external envelope and tail of unconsumed matter or products of combustion, 3, often of a greenish colour.

Olefiant gas issuing with the same degree of force and from the same jet as hydrogen burns with a much larger flame: this is easily explained, as it requires 15 times its volume of air to burn it, whereas hydrogen requires only $2\frac{1}{2}$ times. This shows the influence of quantity on the size of flames. But hydrogen requires only its own bulk of chlorine or of nitrous oxide for combustion, yet its flame in these gases is much larger than it is in air; these burn on the other hand with a very small flame in hydrogen. The only cause to which this can be attributed appears to me to be the difference of their diffusibility. From Mr. Graham's researches we now know the law of the diffusibility of gases,—that it varies inversely as the square root of their density; so that hydrogen, a light gas, diffuses itself much more rapidly through chlorine, than chlorine, a heavy gas, does through hydrogen.

These then are the two causes by which I would explain the different appearances of different flames, and they are confirmed by all the experiments I have made. Probably they are not the only causes, but they are the primary ones, particularly with the simple gases. A great number of experiments were made to ascertain, if possible, the exact proportional effect of these two causes, but with the apparatus I was in possession of, it was a hopeless task. The only way in which I could regulate the jet of gas was by the degree of opening of the stop-cock by which the water was supplied to the gas-holder, a very imperfect method where accuracy is required. With some of the gases produced in the method already described there was not even this resource. The peculiarities of size, &c. which I notice here, are, however, so great and decided, as to leave no doubt about them, allowing freely for inaccuracy of the apparatus.

A few examples may be given in illustration of these remarks. The diffusibility of oxygen to hydrogen is as 1 to 4, or the inverse of the square root of their respective specific gravities (sp. gr. of hydrogen = 1). The difference of size however of their flames is not so great as might be supposed from this compared with chlorine: to account for this it is to be kept in mind that the oxygen requires twice its volume of hydrogen for combustion, so that it must spread further through the hydrogen than it would otherwise require to do, and thus make its flame larger; the flame of hydrogen in oxygen being smaller than it otherwise would be from the same reason. Let us *suppose* it set down in this way:

Vols.	Sp. gr.	Diffusi- bility.	Inverse of quantity.
1	16 oxygen	1	$\times 2 = 2.$
2	1 hydrogen	4	$\times 1 = 4.$

Now let carbonic oxide be treated in the same way.

Vols.	Sp. gr.	Diffus.	Inv. of quant.
1	16 oxygen	3.75	$\times 2 = 7.5$
2	14.12 carbonic oxide	4.	$\times 1 = 4.$

In this case the flame of oxygen should be larger than that of carbonic oxide (that is, when burning in each other), because their diffusibility is nearly equal, and the larger quantity of carbonic oxide is required. On trial I found that it was so in fact; the only instance I have seen with unmixed gases where the oxygen flame is larger than that of the other substance. In the same way the flames of oxygen and sulphuretted hydrogen approximate to each other in size.

The flames of chlorine and hydrogen in each other ought to depend on their diffusibility alone, as their combining proportion is equal volumes. Not having the chlorine in the gas-holder I could not ascertain its size well; but from the trials I have made it is very small, similar to that of oxygen. So also is that of nitrous oxide, and nitrous acid has also a small flame. The flame of hydrogen in these gases is very large, of a greenish or yellowish colour, darkish in centre, 1, (fig. 2) brightest and hottest about the middle of the outer part, 2, but very diffused in its appearance.

The flame of oxygen in hydrogen consists of a dark unmixed central narrow portion, 1, (fig. 3) surrounded by a lilac blue flame of small size, 2, and then by a dark yellowish envelope and tail, 3. The heat is concentrated in the blue portion, where the platinum wire fuses and sparkles brilliantly, the heat being compressed into so small a space.

The flame of oxygen in olefiant gas is a very beautiful and instructive example. It is necessary to premise, that as the oxygen is the least diffusible of the two gases, and as it requires only one third of its volume of olefiant gas to form a proper combustible mixture, both of these circumstances conspire to make its flame small. In this, therefore, we have a small light blue flame, darkish in centre; in this blue flame is the strongest heat which fuses the platinum wire, 1 and 2, fig. 4; surrounding this and stretching far above it was a dull strong yellow flame, *b*, red at the edges and dark in the centre, 3, evidently consisting of solid red hot charcoal, a very large quantity of which was separated as smoke, and adhered to the sides of the flask.

Gases were also mixed with nitrogen and carbonic acid. When a pure gas was burnt in such a mixture it had its flame much enlarged, as it had to expand itself further till it met with a sufficient supply of the other, on account of its dilution. Thus hydrogen, as already noted, burns with a larger flame in air than in oxygen, and oxygen with a very large flame in hydrogen diluted with twice its bulk of nitrogen or carbonic acid. When mixed with the gas which was to issue from the jet an opposite effect was produced; thus the mixture of nitrogen or carbonic acid with hydrogen burnt with a smaller flame in oxygen than pure hydrogen did, and air burnt with a much smaller flame in hydrogen than oxygen did—there being less gas issuing—and of course sooner meeting with a sufficiency of hydrogen. In this case the flame of air was such as represented in fig. 5, burning from a wide hole in a piece of a tobacco pipe: 1, dark; 2, blue very distinct; 3, dark reddish tail; 4, greenish envelope.

Carbonic acid was employed in these experiments, in order that its effects might be compared with those of nitrogen, on the supposition that, on account of its greater density, a jet of gas burning in it would be expanded or diffused to a greater degree than in a similar mixture of nitrogen; this accordingly was found to be the case. The greater degree of contraction which should be supposed to take place when the mixed gases were made to issue from the jet was not demonstrated so clearly by the experiments; in some instances it seemed sufficiently apparent, in others it was doubtful. Experiments with more accurate apparatus would be necessary to establish the proportional effect of such mixtures.

These experiments were made on the idea that from them we should get an explanation of the fact, that carbonic acid is more deleterious to combustion than nitrogen, or “exerts a positive influence in checking combustion, as appears from the fact, that a candle cannot burn in a gaseous mixture composed of four measures of atmospheric air and one of carbonic acid.” (Turner’s *Elements*, sixth edit.). On examining the effect of nitrogen on the combustion of a jet of coal gas, it was found that by successive additions of it to air the flame became more and more expanded, the tube of flame becoming wider and longer, and the white part diminishing or disappearing altogether when the nitrogen was in considerable excess; when beyond a certain proportion, it would not burn at all, obviously from the cooling effect produced by excessive diffusion. It was therefore concluded, that this effect depended on the density of the gases, and accordingly on trial the following results were obtained.

Combustion of coal gas in mixtures of

	Permitted.	Prevented.	Sp. gr. Hydrog. = 1.
1	Oxygen... 7	8	Nitrogen..... 14.12
1	Do. ... 3	4	Muriatic acid ... 18.42
1	Do. ... $2\frac{1}{2}$	3	Carb. acid 22.12
1	Do. ... 2	$2\frac{1}{2}$	Fluosilic acid ... 52.72

Now we observe that their power of preventing combustion is just in the order of their density. Sulphurous acid was also tried, but it was found to hold about the same place that muriatic acid did, which it should not do, as it is denser than carbonic acid, its sp. gr. being 32.1 (hydrog. = 1). It appears, however, that it is decomposed when there is more than a certain proportion of oxygen present to invigorate the combustion; as on inspecting the flame the external blue shell of flame, where the principal combustion takes place, was found to be enlarged and considerably stronger in colour, and a red streaky appearance was also observed in the flame, precisely similar to that observed in the flame of sulphuretted hydrogen, from separation, I believe, of particles of sulphur; this being in accordance with the fact, that hydrogen and carbon at a temperature of ignition decompose sulphurous acid.

Sir Humphry Davy tried the effect of various mixtures of gases in preventing the explosion of oxygen and hydrogen, a table of which he has given. To many of these this explanation does not apply, as the greater part of those he employed were combustible gases; and there is most probably in that circumstance other causes affecting the result, only to be avoided by employing incombustible gases. He has noticed, however, that a wax taper is extinguished in air containing $\frac{1}{10}$ th of silico-fluoric and $\frac{1}{6}$ th muriatic acid gas; also that a larger quantity of steam is necessary to prevent the explosion of oxygen and hydrogen than of nitrogen. This latter fact is usually connected by chemical writers with the heat necessary to maintain steam in the state of gas, but it appears rather to be one instance among the others following the general law; that, *of incombustible gases which remain undecomposed the power of preventing combustion is in the order of their density*: what the exact ratio is I cannot at present say, but beginning with steam, which has a sp. gr. = 9. (Hydrog. = 1), we have the gases increasing in this power in the order already given in the table, namely, nitrogen, muriatic acid, carbonic acid, and fluosilicic acid. The vapour of anhydrous sulphuric acid, sp. gr. = 40.1, should stand between the carbonic and fluosilicic acids, if no decom-

position takes place; this I have not yet tried. *This effect of density in cooling the flame depends on the excessive diffusion of the flame in the denser gas.*

The effect of diffusibility on a flame burning in another gas is obvious enough, for it is visible; but when the gases are mixed together in one vessel its influence is not so easily perceived. It appears to me, however, that in this case it operates in a similar manner: in passing an electric spark through an explosive mixture, or applying a heated body to it, the combination and combustion take place amongst the particles immediately in contact with the exciting cause, and are thence propagated to the rest of the mixture; when the gases are pure, this takes place so rapidly as to appear instantaneous; but if they be diluted with another gas, the progress of the flame may be easily seen. In a mixture which does not explode by the electric spark, I conceive that the particles of the combustible mixture immediately subject to the influence of the spark do combine, but that being diffused to too great a degree, either from excessive dilution or from excessive diffusibility in a denser gas, the temperature is reduced so much that it does not cause combination of the rest of the mixture. This idea seems to be confirmed by the fact (Turner's Elements, sixth edit., p. 252), that "An explosive mixture diluted with air to too great a degree to explode by electricity, is made to unite silently by a succession of electric sparks," namely, from new particles of the mixture coming to be subjected immediately to the action of the spark. This view is likewise supported by other considerations.

In the table I have given fluosilicic acid does not seem to be so powerful as it should be, but this probably depends on impurity. These experiments would require to be repeated on a larger scale, as mine were performed with a jar capable of containing only about 4 oz. of water, and a still smaller trough, with 6 or 7lbs. of mercury.

These researches throw complete light on the action of the blowpipe: the jet of air thrown into the central cool part of the flame is in precisely the same circumstances, and has exactly the same appearance as a jet of air burning in the flask containing a carburetted hydrogen gas, except of course in being surrounded by a hot external flame, and therefore improved by this circumstance. In this case we see the two phenomena at once, the vapour burning in the air, and the air burning in the vapour. Hence also the reason why the flame of the blowpipe is so distinct when thrown through this

part of the flame, and so different when projected on the upper part, where the gas being already mixed with air it can act only as a mechanical agent.

This may be seen very well with sulphur. A crucible of moderate size was filled to about one third with sulphur, and placed on a charcoal fire till it boiled. A jet of oxygen flowing with a certain force was introduced within the upper part of the cavity of the crucible, entirely within the blue burning flame of the sulphur outside; a strong yellowish tapering flame was observed, darkish in centre, and red at edges and tail, of about one inch in length. The same experiment was repeated with the same circumstances, substituting air for oxygen; a very slender jet of blue flame was observed darkish at centre, and red at the tail, greatly smaller than that of the oxygen. These flames are of course expanded by the heat of the vapour to double their volume, but their relative size is in accordance with the principles already laid down.

The influence of quantity is also well seen when diluted gases are burnt from a jet. Thus 1 oxygen and 8 nitrogen or carbonic acid will not burn in the flask filled with hydrogen from the brass jet commonly used, nor even from the tobacco pipe, but will do so from the mouth of the flexible tube about one-fifth inch diameter, forming a thin blue hollow cone of flame, fig. 6; sometimes this cone was deficient at the top, and was extinguished when the gas was made to flow with greater velocity.

In these cases when the diluted gas issues from a small orifice the heat produced from the combustible mixture of the gases is so little that it is cooled by the velocity of the current; from a larger orifice it comes more slowly, and the small quantity of combustible mixture formed constitutes the thin shell of flame observed.

These then seem to be the primary causes regulating the size and appearance of flames, hitherto, so far as I am aware, unobserved or undescribed. Probably, however, these are not the only causes, particularly with compound gases. I have made a few observations on these, and have observed some peculiarities, in nitrous oxide for instance; to these however I shall not advert until they have been examined more particularly. One peculiarity in the flame of hydrogen must have been frequently observed, namely, a green jet inside of the usual dark central portion; and when the flame is full, a dark central part even in this. Having only unsupported conjecture to offer in explanation of this, I shall refrain from saying anything at present. There are

also peculiarities of colour and appearance of the external envelopes and tails so often referred to, which might be worth examining.

These remarks have, I trust, thrown some additional light on the nature of gaseous combustion and the influences affecting it. The inquiry seems to be worth the prosecuting, in order to obtain accurate results. For this purpose superior apparatus is required: one most essential instrument is a gas reservoir, from which gas can be expelled with any degree of force that may be required, and with considerable accuracy. The flask I may also remark, though it may do very well for most flames, does not suit well those which require a large and free supply of the atmospheric gas; thus olefiant gas will scarcely burn in the flask full of air: it shakes violently as if seeking for air, and then goes out; and this cannot be remedied by forcing in air, as the agitation thereby produced blows out the flame. By pursuing the investigation with greater accuracy, results may probably be obtained that may assist in elucidating the nature and affections of gaseous bodies. In the mean time I shall prosecute the inquiry as much as lies in my power, with the view of giving greater accuracy to the results already obtained, or making new observations connected with the subject.

Linlithgow, June 2, 1838.

XV. *Researches on Heat. Third Series.* § 1. *On the unequally Polarizable Nature of different Kinds of Heat.* § 2. *On the Depolarization of Heat.* § 3. *On the Refrangibility of Heat.* By JAMES D. FORBES, Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*

[Illustrated by Plates II. & IV.]

§ 1. *On the unequally Polarizable Nature of different Kinds of Heat.*

IT has been my anxious wish to preserve these papers pure from even the appearance of controversy, and those who have paid attention to the recent history of our present subject must be aware that without making direct allusion to the doubts which have at different times been thrown upon my experiments, I have contented myself with adducing new facts and more convincing reasonings; and I have had the satisfaction to see that the general result of this course has been

* Read before the Royal Society of Edinburgh 16th of April 1838: abridged by the Author from the Transactions of that Society, vol. xiv.; and communicated by him.

the gradual abandonment of such doubts, and the entire adoption of my conclusions.

I believe that only a single exception remains to this statement. I expressed my belief in my *first* paper that heat was differently polarizable, according to the source whence it was derived. M. Melloni* failed to verify this result; and the opposite conclusion, namely, that all kinds of heat are equally polarized by a given pile of mica, was prominently put forth by himself and M. Biot as an important discovery†. Without any undue confidence in my first, confessedly imperfect, researches, I proceeded in my second paper‡ to give what I considered ample proofs of the correctness of the statement, though the great dissimilarity of the numbers arrived at from those of my first paper, showed that the latter were worthy of very little confidence on the ground of numerical exactness, which, indeed, I never claimed for them. The later experiments, however, were made with a view to accurate results, and I stated certain forms of the experiment which I had devised on purpose to meet the objections of M. Melloni, although I avoided mentioning his name.

It seems, however, that M. Melloni, returning to the subject with his accustomed diligence, after receiving my second paper, still confirmed his former results, and he has attempted to show, in a very long paper, published in the *Annales de Chimie* for May 1837 (which only appeared in October), that his results must be exact, and the probable source of my errors. I contented myself with giving a very brief answer to this paper in the *Philosophical Magazine* for December 1837, admitting the improbability that so experienced an operator as M. Melloni should be wrong in his numerical results, but stating convincing grounds for believing that his explanation of my conclusion, founded on experimental errors, was inapplicable. The inquiry which I have since been led to make, and the entirely satisfactory explanation at which I have arrived of a difference so puzzling, terminating in a confirmation of my original statement, I now proceed to detail.

I have not the remotest intention of examining and criticising M. Melloni's paper in the *Annales de Chimie* for May 1837, as respects trifling or personal matters, which I readily confide to the impartiality of those best qualified to judge: but it is quite necessary to state the facts which I had observed, and M. Melloni's mode of accounting for them.

With two polarizing mica bundles of great tenuity, pre-

* *Comptes Rendus de l'Académie des Sciences*, ii. 140.

† *Ibid.* p. 194.

‡ *Lond. and Edinb. Phil. Mag.*, vol. xii. p. 549 *et seq.*

pared in the method described *, marked I and K, I found that, with heat from an Argand lamp, 72 to 74 per cent. of the incident rays were polarized, that is,—of 100 rays transmitted when the plates were parallel, 72 to 74 were stopped when one was *crossed* or its plane of refraction turned through 90°. With heat from boiling water, but 44 per cent. were polarized, and heat from sources of intermediate intensities gave intermediate results.

M. Melloni ingeniously argued that this appearance might arise from the circumstance that the mica bundles becoming most heated by those kinds of heat which they absorbed most readily, or transmitted least easily (*viz.* heat of low temperature), the pile was continually receiving a supply of heat by secondary radiation from the mica, which, having no relation to the *parallel* or *crossed* positions of the plates I and K, of course tended to diminish the apparent polarization of the heat, or to equalize the effect in the two positions.

The supposed effect of secondary radiation from plates had been so often urged against my experiments, that, though as often proved to be insignificant or insensible, it gave me no surprise to see it started afresh, and in so plausible a manner. M. Melloni was probably not aware that the screen for intercepting the heat was placed *between* the source of heat and the polarizing plate K, (as shown in Pl. II. fig. 2,) so that the mica plates were only absorbing heat during the exceedingly short time (10 seconds) of one swing or dynamical impulse of the needle, otherwise I do not think he would have urged so infinitesimal an objection†. I endeavoured, however, to meet it directly in this way. I took two mica bundles, G and H, and placed them parallel, as shown in fig. 3. But instead of placing the pile at P, where it receives at once the directly transmitted heat from S (the screen being removed), and the supposed secondary radiation of the surface *ab* of the mica plate, I placed it at *p*, identically situated with respect to the surface *ab*, but wholly removed from the influence of direct radiation from S. When this experiment was performed with dark heat (which, according to Melloni, ought to give the greatest effect) not the slightest movement of the galvanometer-needle was observable on removing the screen, during a far longer space of time than is ever in practice allowed for the absorption of heat. This experiment ought to be considered quite conclusive.

* Lond. and Edinb. Phil. Mag., vol. xii. p. 550.

† I might add, too, that, had he been aware of the extreme tenuity of the mica plates employed (of which more hereafter), he must have been led as a necessary consequence of his own reasonings to admit that the effect must be insignificant.—*Ann. de Chimie*, Mai 1837, p. 13, note.

M. Melloni had hinted that the different dimensions of the sources of heat, and the various angles under which the rays fell on the mica plates must materially affect the results; and, as I was quite convinced that operating with parallel rays was the most correct method, I proceeded to repeat my experiments on his plan, with a salt lens placed in front of the source of heat so as to render the rays parallel; I also removed the polarizing and analysing plates to a considerable distance from the pile, and afterwards varied their distance in order to see whether any adequate explanation of the discrepancy could thus be obtained.

The apparatus was arranged in the following way:—A rock-salt lens was placed between the source and screen (fig. 2 above) so that the heat was refracted into a nearly parallel beam before incidence upon I and K, which were removed to a distance of more than a foot from the pile; the distance of the source being 2 feet.

The apparent polarization was somewhat increased, as I had anticipated, from the rays falling more nearly at a constant angle when previously rendered parallel; but the different polarizability of the different kinds of heat was even more distinctly marked than ever; whilst the distance of the mica plates from the pile was now such as to reduce to insignificance any effect of secondary radiation, had such before been sensible.

In prosecuting these experiments, most of which were repeated many times under various circumstances, I remarked more distinctly than formerly the influence of particular states of combustion of the source of heat upon the index of polarization, and the accidental variations to which this gives rise on different days, and even during the progress of an experiment. Heat from brass about 700° I have generally found the most uniform on different days, though there occasionally occurs in a series of experiments, considerable deviations from the mean. The Locatelli lamp seems subject to greater variations, and the Argand still more; indeed, I have found it so impossible to maintain an Argand lamp in a uniform state of combustion, even for a quarter of an hour, that I have lately abandoned the use of it. But the quality of the heat from incandescent platinum varies between the widest limits. Nor is this wonderful; it is composed of heat from two very different sources combined in uncertain proportions, that from the incandescent coil of wire, and that from the alcohol flame which heats it. The intensity of incandescence, too, varies exceedingly. On one occasion, when the incandescence was unusually bright, and the alcohol flame very

low, I obtained a higher degree of polarization than I have ever done before or since. The ordinary proportion between the indications with I and K *parallel* and *crossed*, is with incandescent platinum 100 to 26 or 27. In this case it was 100 : 20 ; and when the heat was lifted by an interposed plate of thin glass, it rose as high as 100 : 13.

The general results obtained in the way above described are stated in the following table, in which I have included the numbers for mercury heated to 410° , and for boiling water taken from the second series* ; those experiments not having been repeated because the use of a lens is in those cases of little avail.

Polarizing Plates I and K.

Source of heat.	Rays out of 100 polarized.
Argand lamp	78
Locatelli lamp	75 to 77
Incandescent platinum (usually)	74 to 76
Incandescent platinum, with glass .06 inch thick, interposed, 6 to 7 per cent. more, or	80 to 82
Alcohol flame	78
Brass heated to about 700°	66.6
Ditto, with a plate of mica .016 inch thick interposed, (between I and B)	80
Mercury in a crucible at 410°	48
Boiling water	44

I presume that it will be conceded, that the experiments now cited, incontrovertibly establish the unequal polarizability of heat from different sources. Yet, I confess, I should have felt uneasy, could I have thrown no light upon the cause of the discrepancy between M. Melloni's results and my own. This I believe, that I am able completely and satisfactorily to do, allowing him every credit for the perfect exactitude of his experiments. For the sake of clearness, I will state the course by which I myself arrived at this result.

It occurred to me, that it would be satisfactory for the further and independent confirmation of the conclusions just given (which were then only partially obtained), to examine the index of polarization (by which I mean the per centage of the heat stopped in the crossed position of the polarizing and analysing plates) deducible for different sorts of heat, from a series of experiments made wholly without reference to this question, I mean those on depolarization, considered in another section of this paper, and which, it will be seen by

a reference to the mode of reduction there employed, required to be recomputed in order to give the index of polarization.

I at first imagined, that the experiments made with each of the three kinds of heat then employed (Argand lamp, incandescent platinum, and dark hot brass) would give throughout the same result for the same kind of heat. This was far from being the case; the interposition of the depolarizing plate of mica between the polarising and analysing plate, acting simply by transmitting only certain rays of heat, had modified the index of polarization, and that more or less, as the thickness of the interposed mica was more or less considerable. Such a result might have been anticipated, as in exact conformity with the discovery I had formerly made; but I was misled by a false notion, which I had heedlessly adopted, and suffered to remain unquestioned, that, in order to affect the index of polarization, the heat must have been modified by transmission *previous* to its falling upon the first or polarizing plate, whilst, in the experiments referred to, the modification took place between polarization and analysis*. Of course, when I perceived this oversight, the confirmation of my views was greater, because it was unforeseen.

But the most material result of the examination of those experiments was this. By a reference to the section on depolarization, it will be seen that five different thicknesses of mica (varying from three to sixteen thousandths of an inch) were interposed successively, and the index of polarization determined for each of the three kinds of heat. Now, upon examining the result of these fifteen experiments, I clearly perceived (amongst occasional irregularities) this law to prevail,—*that whilst a film of mica .003 inch thick scarcely altered the characteristic properties of heat from different sources, as shown by their variable indices of polarization, an increased thickness of mica had almost no sensible effect upon the heat from the Argand lamp, but it increased the index of polarization of dark heat so fast, that, with a thickness of mica of .016 inch interposed, the apparent index of polarization for heat from the Argand lamp, incandescent platinum, and dark hot brass, was almost the same.*

When I had fully seized this conclusion, the explanation

* Lest this confusion should, by possibility, occur to any one, as it did to myself, I will observe that the position of the sifting or modifying plate, absorbing the least refrangible rays, is quite immaterial, provided it occur between the *source* and the *indicator* of heat; for whether the rays in question are absorbed before or after polarization, those which ultimately escape and reach the pile are the only ones of which the index of polarization is measured.

of M. Melloni's results was easy and complete. It appears from the account of his experiments, that he still employs piles of mica of the form I at first used, consisting of distinct laminae separated by a knife, then laid together and united at the edges, up to the number of 30, 60, and even more*. On the other hand, the piles I and K, which for two years and a half I have employed, are of a degree of tenuity really surprising. The mode of their construction I mentioned briefly in my last paper, art. 20, and it is so very superior to any other, that it is probably from inadvertence that it has not been generally employed. The piles laminated by the action of violent heat, afford a multiplicity of parallel surfaces in a given thickness of mica, which no mechanical method can approach. The actual thickness of mica which they contain, I am unable accurately to estimate. The plates marked G and H are much thicker, perhaps twice as thick as those marked I and K, which I commonly use; yet the former, as I roughly estimate by the tint they give in polarized light, are only about one thousandth of an inch in thickness. At the utmost, the plates I and K can be but one fifteen hundredth of an inch; and yet it appears that their polarizing power (depending solely on the number of surfaces they contain) is equal to M. Melloni's pile of ten distinct plates placed at the same angle (35° to the incident rays). The mean thickness of the elementary plates can, therefore, be only one fifteen thousandth of an inch; and they reflect abundantly the colours of Newton's rings.

Now, I have found by the depolarization experiments, that it requires a much greater thickness of mica than that traversed by the heat in passing through the plates I and K (even allowing for the obliquity) to affect materially the index of polarization of heat from different sources, such as from brass at 700° , and incandescent platinum. It is, therefore, a

* *Annales de Chimie*, Mai 1837. At p. 17, &c. M. Melloni has given a minute account of that method of constructing the piles, which, "amongst several different ways, he considers the preferable one." No one could doubt from his language that he is describing a new and improved form of the apparatus. I regret for a moment to descend to notice an apparent want of justice and courtesy towards myself; but it is impossible for me to not to observe, that the procedure he so exactly details, is, to almost the minutest particular, identical with that which I myself used in June 1835, in constructing, in M. Melloni's presence, the first pair of piles used for polarizing heat which existed in France, at a time when M. Melloni expressed his unqualified scepticism as to the polarization of heat generally; which piles I left, at his desire, where I presume they now are,—in his own possession. This mode of construction I soon after abandoned, for the improved one alluded to in the text.

necessary consequence of the construction, that the heat passes through such piles as I use unaltered, or nearly unaltered, in its character, whilst in passing through bundles of detached plates laid together, the thickness of mica to be traversed is sufficient to modify the heat by absorption, in such a way that *the difference of quality has vanished, whatever be the source, in the very act of transmission.* It is hardly likely, considering the size of M. Melloni's mica plates (4 inches long and 2 wide), that they could be less than one fifteen hundredth of an inch thick each; a pile of ten would then be ten times as thick as my pile of equal energy, and at an incidence of 55° the thickness traversed would not be much shorter than that of the mica plate alluded to in art. 20, which we have there seen to be sufficient to obliterate all distinctive character as to polarizability between an Argand lamp and dark heat.

Being now fully aware of the importance of the construction of piles of mica which I had adopted, I thought it worth while to examine the proportions of heat from different sources, which these very delicate laminæ were capable of transmitting, which, I presumed*, would be found far less variable than when plates of the usual thickness are employed. My expectations were more than realized, as is seen in the following table, the second column of which shows the proportion, to the whole incident heat, of that transmitted by the two mica piles I and K placed parallel to each other; by far the greater proportion of the loss being that due to the obliquity of reflection and the number of surfaces†. By way of contrast, I have placed in the third column the proportion of the whole incident heat transmitted at a vertical incidence by a plate of mica $\cdot 016$ inch thick.

Source of Heat.	Rays out of 100 transmitted by	
	Plates I and K parallel.	Mica plate $\cdot 016$ inch thick.
Locatelli lamp	18·8	57
Ditto, with plate of glass $\cdot 06$ inch thick interposed . . }	16·2	72
Incandescent platinum . .	17·6	50
Dark hot brass (700°) . .	15·5	15
Heat from boiling water . .	10·	8

It is very evident that, for the first four sources of heat at least, the transmissive power of the plates I and K varied

* I do not state this as a new idea; it has been repeatedly remarked by M. Melloni, that, in proportion as substances are thinner, they possess a more equable diathermancy for heat of different qualities.

† The part of the effect due to reflection, I had previously established to be nearly the same for different kinds of heat.

little, and in no sort of proportion to the characteristic action of mica even in moderate thicknesses. This will be more evident, if we compare the ratios of the heat from different sources transmitted in the two cases, taking the heat from the lamp sifted by glass as the standard for each column.

	Plates I and K.	Mica .016 inch.
Locatelli with glass . . .	100	100
Locatelli	116	79
Incandescent platinum . .	108	70
Brass at 700°	96	21
Heat of 212°	62	11

I need hardly add, that so remarkable a result as that the heat sifted by glass should be less readily transmitted by the thin mica laminæ, than the direct heat from a lamp, was carefully verified.

Since, then, the first four kinds of heat are transmitted without any great difference of proportion, by the piles I and K, and since, especially, the heat from a lamp sifted by glass and that from dark brass possess almost exactly similar characters in this respect, it is very clear that we have a new ground for rejecting as untenable M. Melloni's supposition, that the apparent differences of polarization in my experiments, arose from the unequal proportions of heat absorbed by the mica piles when the source varied.

Admitting, then, the fact of the variable index of polarization exhibited by heat of different qualities similarly treated, we are tempted to inquire what explanation can be offered of it. This question, inferring for its answer a knowledge of the nature of heat, we are not prepared to answer with confidence. My former suspicion of its being due solely to the difference of the refractive index of mica for heat of different kinds, I am disposed to retract as inadequate, or at least to suspend my judgement respecting it. I at one time thought, that, supposing the mica bundles unequally permeable to heat from different sources, a difference of ratio in the total heat reaching the pile with the plates I and K. *parallel* and *crossed* might be accounted for. But a careful analysis of the circumstances convinced me, that the absorptive action, if assumed the same for common and polarized heat, could produce no such effect. One of the most plausible suppositions which occurred to me was this,—that, supposing the reflection of luminous heat to take place more copiously at the mica surface than that of dark heat, and supposing the angle of incidence to be that of total polarization, since the refracted ray contains as much heat (if heat be like light) polarized *perpendicular* to the plane of incidence, as is reflected and

polarized *in* the plane of incidence, the ratio of the polarized to the total heat transmitted would be greatest in the heat of highest temperature. Unfortunately for this theory, careful experiments assured me that heat from different sources underwent the same, or nearly the same, intensity, of reflection under the same circumstances.

We are, therefore, led to regard this character of unequal polarizability, as probably indicating a difference of character of a fundamental kind between heat and light; at least a super-added quality or peculiarity of vibration, which becomes more and more sensible as heat is removed in its character from light, or has (as we shall hereafter see), generally speaking, a lower degree of refrangibility. A sensible undulation, normal to the surface of the wave, would of course satisfy this condition. I am far from saying that my experiments warrant such a conclusion. I am aware that it is inconsistent with the ideas entertained by some ingenious speculators upon the nature of heat *; but this very circumstance has led me to bestow the greater pains upon establishing the phænomenon in an incontrovertible manner.

§ 2. *On the Depolarization of Heat.*

In the first series of these researches, § 4, I entered pretty fully into the subject of depolarization. The establishment of the fact was of the highest importance, since there is little probability of proving in any more direct manner the doubly refractive energy of crystals with respect to heat. But, besides the demonstration of the fact, I pointed out in that paper the important numerical determinations to which it might lead; determinations of the first consequence to the theory of heat, and the discrimination of heat from light. The measure of depolarization in the case of light, or the quantity of light which has become polarized in a new plane by passing through a doubly refracting plate, such as mica, depends, 1. upon the length of a wave of light; and, 2. upon the retardation which one of the doubly refracted pencils of light suffers, upon the other, in passing through the mica, which retardation differs with the material of the plate, varies directly as its thickness, and *may* also vary with the quality of the incident ray.

Hence, as a little reflection clearly shows, if the quantity of light (or, by analogy, of heat) depolarized by a plate of given thickness be numerically estimated, we may, if the length of the wave be given, determine the retardation, or energy of

* Kelland on Heat, art. 166.

double refraction; or, if the latter be assumed or known, we may find the length of a wave. Considering the latter element as the more important, and not being then in possession of any more direct mode of determining it numerically, I proposed to *assume* the retardation due to double refraction as the same for heat as in the case of light, (considering heat as but less refrangible light), and to determine the length of a wave in the way which I fully explained in the First Series, art. 68-75*.

Two circumstances require notice by way of precaution. The first is, that, for the very reason that we have periodical colours in the case of light, there are *different* thicknesses of mica and different measures of retardation, which, for the same length of a wave, will give the same measure of depolarization; these dubious cases (which the formula of depolarization completely expresses) must be distinguished. The second remark is, that all our sources of heat furnishing heterogeneous rays, each has its own period of maximum and minimum intensity, just as in the case of solar light, and since our means of numerical estimation embraces the sum of all the effects of heterogeneous rays, we cannot expect results which shall rigorously satisfy a formula, in which homogeneity (or constancy of λ , the length of a wave), is assumed, but consider the approximate result as representing the mean or predominating character of the heat employed.

Recalling, then, Fresnel's formula, quoted in art. 70 of the First Series†, we have

$$\frac{E^2}{F^2} = \sin^2 180^\circ \left\{ \frac{o-e}{\lambda} \right\}$$

where F^2 is the intensity of the whole incident polarized ray; E^2 the intensity of that portion which, after transmission through the depolarizing plate, is capable of being analysed in a perpendicular plane. These two quantities being determined from observation, the first side of this equation, or their ratio, becomes known. On the second side we have two quantities, either of which may be assumed, and the other becomes known, viz $o-e$ the retardation of the one doubly refracted ray upon the other within the crystal, and λ the length of a wave. Now, it is obvious from the form of the expression, that an infinite number of values of $\frac{o-e}{\lambda}$ will satisfy the equation; in light there can be little ambiguity arising from this cause, because the phenomena of periodic colours at once afford the means of selecting the true solu-

* Lond. & Edinb. Phil. Mag., vol. vi. p. 366.

† *Ibid.*, p. 367.

tion. In the case of heat, we must proceed with more caution, the value of $\frac{o-e}{\lambda}$ being wholly unknown; we only assume (as we are entitled to do) that this quantity increases uniformly with the thickness of the plate, which it necessarily must, since the retardation is as the thickness, and λ is independent of it. By a very simple process, the true value was easily selected.

Five depolarizing mica plates, of different thicknesses, of exactly the same quality, and each as uniform as possible, were provided. They were cut to the same size, and of such a form that each could at once be placed with its neutral axis (a line in the plane passing through the two axes of double refraction) vertical, or inclined 45° at pleasure. Their thickness was next to be determined. The examination of the colours shown by polarized light was the most obvious method, but not susceptible of the exactness which was required. It was, however, used as a check.

The following were the results of actual measurement, made by means of a pair of callipers constructed for such purposes by Troughton. These results are the mean of ten measures each, which were rendered difficult by the elastic and fissile nature of the substance.

	Thickness in parts of an inch.
No. 1.	·0026
No. 2.	·0044
No. 3.	·0074
No. 4.	·0060
No. 5.	·0157

With these mica plates in succession, employed for depolarizing, I proceeded to determine the ratio $\frac{E^s}{F^s}$ (p. 107) for the most part exactly in the way described and illustrated by an example in art. 71, First Series, which I found preferable to any other. This laborious investigation I performed for heat from three sources; (1.), an Argand lamp with glass chimney; (2.), incandescent platinum; and, (3.), brass heated (not to visible redness) by an alcohol flame. The thickness of the plates No. 3. and No. 4. being very nearly the same (and giving, as they ought to do, almost exactly the same measure of depolarization), I preferred using the united thickness of Nos. 2. and 3. as an interpolation between Nos. 3. and 5. The swings of the needle, or dynamical effects, vol. xii. p. 547, were always observed, and are alone given. The polarizing and analysing plates were the same marked I and K, before fully described (vol. xii. p. 550), and a plate is said to

be at 0° or at 90° as its plane of refraction is vertical or horizontal. With these explanations, and a reference to art. 71, First Series, the following specimens of observations will, it is hoped, be intelligible.

Argand lamp: 16 inches from centre of pile, depolarizing mica plate No. 3.

Position of Polarizing Plate K (I being always at 0°).	Position of Neutral Section of Mica.	Galvanometer. Dynamical Effect.	Total Polarization F ₂ .	Depolarization E ₂ .
At 0°	At 0°	11.9	8.45	+5.35
At 90°	—	3.45		
—	At 45°	8.8	—
At 0°	—	6.75	8.35	-5.35
—	At 0°	12.1		
At 90°	—	3.75	+5.05
—	At 45°	8.8	—
At 0°	—	6.7	8.35	-5.35
—	At 0°	12.05		
At 90°	—	3.7	—
Means			8.38	5.27

The following experiment was made with heat wholly unaccompanied by light, and with the same mica plate.

Dark hot brass: 14 inches from pile, depolarizing mica No. 3.

Position of Polarizing Plate K (I being always at 0°).	Position of Neutral Section of Mica.	Galvanometer. Dynamical Effect.	Total Polarization F ₂ .	Depolarization E ₂ .
At 0°	At 0°	5.25	[3.25]*	+3.75
At 90°	—	2.0		
—	At 45°	5.75	—
At 0°	—	2.15	4.0	-3.8
—	At 0°	5.95		
At 90°	—	1.95	+3.8
—	At 45°	5.75	—
At 0°	—	2.6	3.95	-3.3
—	At 0°	5.9		
At 90°	—	1.95	+3.7
—	At 45°	5.65	—
At 0°	—	2.3	4.0	-3.5
—	At 0°	5.8		
At 90°	—	1.8	—
Mean			3.98	3.64

* Omitted in the mean as manifestly too small, arising from the lamp being just lighted, and the brass not fully heated.

It now remains to explain how these observations have been discussed. The ratio $\frac{E^2}{F^2}$ is at once obtained by dividing the second mean result by the first, and I have purposely quoted these observations, to show how very nearly the plane of polarization was thrown at right angles by the action of this particular thickness of mica, especially in the case of dark heat, which appears to be owing to its greater homogeneity, as we shall presently have reason to infer.

We have seen above (art. 32) that

$$\frac{E^2}{F^2} = \sin^2 180^\circ \left\{ \frac{o-e}{\lambda} \right\}$$

And therefore,

$$\frac{o-e}{\lambda} = \frac{\sin^{-1} \sqrt{\frac{E^2}{F^2}}}{180^\circ}$$

Since the radical has an ambiguous sign, the equation will be satisfied by a value of $\frac{o-e}{\lambda}$ equal to a fractional number a , or by $1-a$, or $1+a$, or $2-a$, or $2+a$, or $3-a$, &c. In the case of the two examples given above, we have for the Argand

$$\frac{E^2}{F^2} = \frac{5.27}{8.38} = .629; \sqrt{\frac{E^2}{F^2}} = \pm .793$$

And $\frac{o-e}{\lambda} = .29$ or $.71$ or 1.29 or 1.71 , &c.

For the dark heat, $\frac{E^2}{F^2} = \frac{3.64}{3.98} = .915; \sqrt{\frac{E^2}{F^2}} = \pm .957.$

And $\frac{o-e}{\lambda} = .41$ or $.59$ or 1.41 or 1.59 , &c.

The true value must be such, that, when a number of plates are employed, $\frac{o-e}{\lambda}$ must increase *uniformly* with the thickness of the plates.

Clearly to mark this, and at the same time to combine the results by graphical interpolation, I projected the numbers obtained as above in the way shown in Plate II. fig. 1. On a horizontal line spaces representing the thickness of the plates were set off as abscissæ, and a few of the ambiguous values of $\frac{o-e}{\lambda}$ as ordinates, which are marked by dots. It was then easy to select those points thus set off, through which a straight line could most nearly be drawn, representing the

linear relation between the thickness of the plate and the quantity $\frac{o-e}{\lambda}$, (both vanishing when the thickness = 0,) and inspection of the figure will show that no doubt can attach to the choice of the ambiguous numbers, and also that the straight line represents in general remarkably closely the course of those points.

There is one exception to this statement, and it is an important one. It will be observed that the interpolating line, instead of passing through any of the dots set off for the mica plate No. 3, bisects exactly *two* dots. These dots are nearest to one another in the case of dark heat,—wider apart with incandescent platinum, and widest of all in the case of the Argand lamp. The explanation is complete and satisfactory. The interpolating line in all these cases gives a value of

$$\frac{o-e}{\lambda} = \frac{1}{2}, \text{ which gives a value of } \frac{E^2}{F^2} = 1; \text{ in other words,}$$

infers a *total* polarization of the heat in the horizontal plane (or in the case of light total darkness, when the polarizing and analysing plates are parallel) which we know can only occur when the heat is absolutely homogeneous. The want of mathematical coincidence in this case infers the admitted physical condition of want of homogeneity in the incident rays. Hence, we infer that dark heat is most homogeneous; next, that from incandescent platinum; and, least of all, that from the Argand.

The proportions give numerical results almost identical for the three sources of heat; a result so far contrary to what I expected, that it shows that by this method we cannot hope to discriminate the different lengths of waves of these kinds of heat, as I had formerly supposed, and shows that the variation of λ must be very small, or else (what is improbable) that it is constantly proportional to the variation of the retardation $o-e$.

In all the three cases we have as nearly as possible a value of

$$1.4 \text{ for } \frac{o-e}{\lambda} \text{ at a thickness of depolarizing mica, equal to}$$

·020 inch, or ·07 for a thickness of ·001 inch. Let us compare this with the case of light. The sum of the retardations for the various mica plates, as given in art. 33, amounts to ·000199 inch; the sum of the thickness in the next article is ·0361 inch, consequently the mean value of the retardation or $o-e$ is ·0000055 for a thickness of mica of one thousandth of an inch. But the length of λ for extreme red is ·0000266,

for extreme violet, $\cdot 000167$ inch. Hence for a plate of mica $\cdot 001$ inch thick the values of $\frac{o-e}{\lambda}$ are

$$\text{For extreme red light} \dots \frac{55}{266} = \cdot 207$$

$$\text{For extreme violet light} \dots \frac{55}{167} = \cdot 329$$

$$\text{For heat} \dots \dots \dots = \cdot 07.$$

If we assume the retardation, or $o-e$, to be the same for all lengths of waves, and for heat as for light, we immediately deduce the value of λ , or the length of a wave of heat.

For since for a plate $\cdot 001$ inch thick, $\frac{o-e}{\lambda} = \cdot 07$, as above, $o-e = \cdot 0000055$, we have

$$\lambda = \frac{o-e}{\cdot 07} = \frac{\cdot 00055}{7} = \cdot 000079 \text{ inch,}$$

about three times as long as a wave of red light, and four and a half times that of violet. But it is always to be remembered, that this proceeds on the supposition of the retardation being invariable.

I have taken the trouble to calculate and project in a similar manner my original observations on depolarization given in the First Series of these researches, art. 74, in order that, though probably less accurate, they might form a check upon the results just given. The plates *then* employed, and marked No. 1 and No. 2, (which are not to be confounded with those so designated in this paper) had thicknesses (deduced from the retardation) of $\cdot 0072$ and $\cdot 0036$ inch. I have the gratification to find that the computed results agree almost precisely with those just obtained, although from the accidental thicknesses of the two plates employed the observations with *these alone* do not enable us to select the appropriate value of $\frac{o-e}{\lambda}$, there being at least two values which still remain ambiguous; but when taken in conjunction with the observations of art. 41, the ambiguity is at once removed, and the numerical value of λ comes out almost exactly as stated above, for incandescent platinum and dark heat, and somewhat smaller for that of the Argand lamp.

I desire it to be recollected, that, in speaking of these somewhat startling lengths of waves of heat, I am using the language of only one of the two hypotheses which serve to interpret the results of this section; for, if the variation be in $o-e$, or the difference of the velocities of the doubly reflected

rays in mica, the result would be the same. The experiments in a subsequent part of this paper may serve to guide us in our choice. Meanwhile, I would observe, that, supposing the above results to be explained on the supposition that $o-e$ is smaller, instead of λ greater for heat than for light, it is equivalent to supposing the doubly refracting energy weaker, or a greater thickness of a crystal required to produce a given effect. Our suggestion respecting the existence of sensible vibrations normal to the wave surface (art. 28) will not avail us here. For, by the mode of reducing the experiments on depolarization, the unpolarized part of the heat does not enter into consideration at all*; consequently those parts of the total effect which are due to transverse vibrations alone, are not modified by double refraction as so much light would be.

[To be continued.]

XVI. *On a new Compound, consisting of Iodide of Potassium, Iodine, and the Essential Oil of Cinnamon.* By JAMES APJOHN, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Ireland.†

THE compound which is the subject of the present communication owes its origin to an unchemical medical prescription. A solution of iodine and iodide of potassium in cinnamon water having been directed by a physician of this city in the winter of 1837, his patient found that during the prevalence of very cold weather, the solution, which had been previously turbid, became quite clear, and nearly insipid, and upon examining the bottle closely he observed deposited in the bottom a small quantity of minute capillary crystals. These crystals were brought to Mr. Moore of Anne-street, the apothecary in whose establishment the prescription was made up, and by him to me for chemical examination and analysis. Before detailing the means which I have employed for determining the exact constitution of this substance it will be proper to give the process by which it is best procured, and enumerate its leading properties; points, both of which were investigated by Mr. Moore and myself conjointly.

* I do not mean to offer any opinion on the nature of light in a partially polarized ray generally; but, as in the present case, the angle of incidence is that of complete polarization nearly, I presume that the transmitted ray is undoubtedly composed partly of light polarized perpendicularly to the plane of incidence, and partly of common light.

† Communicated by the Author.

To a gallon of cinnamon water*, first reduced nearly to 32° , add four ounces of iodide of potassium and forty grains of iodine previously dissolved in a minimum of cold water. Upon the instant of admixture the solution becomes quite turbid, owing to the production of a yellowish sediment, and this in less than a minute becomes crystalline, and then gradually subsides. The supernatant solution, which appears almost entirely deprived of iodine and oil of cinnamon, is now drawn off with a siphon, and the crystals and residual fluid thrown upon a single filter, which, when sufficiently drained, is enveloped in several folds of blotting-paper, and transferred to a chalkstone, where, by the absorbent powers of the latter and the occurrence of spontaneous evaporation, the product is rendered perfectly dry and pure. With the quantities stated above 60 grains of the compound are obtained. A temperature at or very close to 32° is necessary to the success of this process. At 40° the brown powder already noticed is alone produced, and in much diminished quantity. This brown sediment, however, is identical with the crystalline product, for it may be converted into crystals simply by reduction of temperature, and I have even found it to undergo the same change when collected on a single filter, and set to dry on a bibulous stone at the temperature of 45° .

The crystals are capillary quadrilateral prisms, without pyramidal terminations. They are of a beautiful brown or bronze colour, and have a strong metallic lustre. Their taste is extremely hot and pungent, resembling closely that of oil of cassia, but partaking also of that of iodine. In alcohol and æther they are readily dissolved, and from these solvents they are again deposited with their original appearance upon the occurrence of spontaneous evaporation. They are decomposed by water, which extracts from them iodide of potassium, and causes the separation of oily drops of a dark colour, which are either a mechanical mixture or a peculiar compound of iodine and the oil of cinnamon. This action of water, however, is greatly diminished when it is close to the freezing point, and appears altogether prevented when a certain amount of iodide of potassium is present.

When heated to 82° the crystals melt into a dark liquid, from which upon cooling the original substance is reproduced. When heated beyond its melting point iodine and a vapour smelling strongly of oil of cinnamon sublime, and iodide of potassium is left behind, mixed usually with a little carbon resulting from the decomposition of a portion of the oil.

* This water should be prepared by introducing into a still one pound of cassia bark and two gallons of water, and drawing off one gallon.

Starch would appear to decompose this substance, for with even its alcoholic or æthereal solution it forms the well-known blue compound. When agitated with water and zinc or iron filings, an iodide of these metals is produced, and the oil is set free. With mercury the result is the same, and in each instance for water alcohol or æther may be substituted. Potash also at once developes the oil, forming, as in the case of free iodine, iodide of potassium, and iodate of potash.

From these facts it seems legitimate to infer that it is the oil, and not any modification of it corresponding to the benzoyle of chemists, which is associated with the iodine and iodide of potassium, and that they are all held together by an extremely feeble affinity, in as much as not only is the iodide of potassium separated by water, as has been stated, but the iodine is affected by a solution of potash just as if it were free. To test the truth of this opinion, a little of the compound was decomposed in a small glass retort by the exact equivalent of a very dilute caustic alkali, and, a receiver being applied, about half an ounce of a liquid having the appearance and obvious properties of cinnamon water was drawn off by distillation. From it, however, I could not, though every precaution was employed, procure a particle of the original crystalline compound. The properties, indeed, of the distilled liquid were not, upon an accurate examination, identical with those of cinnamon water. Its odour, for example, was slightly different, and it reddened litmus, a circumstance from which it may be inferred to contain cinnamic acid. It is therefore not unlikely that the oil may have absorbed oxygen or have been otherwise altered during the distillation; and as a confirmation of this opinion I may mention that the oil of cassia which is found in the market, is chiefly cinnamic acid, and that a cinnamon water prepared from it by a process directed in some of the pharmacopœiæ yields but a very minute proportion of the substance which is the subject of the present paper.

With a view to the analysis of this compound the first point to determine was the proportion of iodide of potassium which it included. To accomplish this a known weight of it was heated in a small porcelain capsule, by which iodine and oil of cinnamon were expelled in the vaporous state, and there remained a mixture of iodide of potassium with a little carbon resulting from the decomposition of a portion of the oil. The iodide of potassium was separated from the carbon by solution in water, and the use of a single filter which had been previously deprived of all soluble matter by the action first of a dilute acid, and subsequently of distilled water. The

filter being well washed, the solution was evaporated to dryness in a carefully counterpoised capsule, and then accurately weighed. The following are the results of three experiments thus conducted.

	I K	I K (per cent.)
3.37 grains gave.....	0.43	12.75
8.00	1.03	12.87
9.40	1.13	12.02

The mean therefore of the numbers in the third column, or 12.55* is the quantity of iodide of potassium as obtained by me in 100 grains of the compound.

The next step was to investigate the iodine associated not with the potassium but with the oil, and to effect this the following was the course first pursued.

A known weight of the compound was decomposed by a slight excess of an alcoholic solution of potash, and the whole was evaporated to dryness, by which the oil was partly volatilized and partly decomposed. Heat was now cautiously applied, so as to reduce the iodate, which I have already stated to be always formed in such experiment, to the state of iodide of potassium, but not to volatilize any of the latter salt. The residue, first permitted to cool, was treated with distilled water, and passed through a filter to separate the carbon. The filter was well washed, and the solution, having been reduced to a small bulk by evaporation, was precipitated by nitrate of silver, and the iodide of silver, firstedulcorated three or four times with cold distilled water containing a few drops of ammonia, was finally dried, melted and weighed.

In an experiment in which 10.33 grains of the compound were employed, the iodide of silver amounted to 7.41 grains, equivalent to 3.95 of iodine, or 38.24 for 100 grains of the compound. Now, if from this we subtract 9.58, the iodine in the 12.55 grains of iodide of potassium which we have already found to exist in 100 of the compound, we shall get for the per centage of iodine in union with the oil the number 28.66.

Fearing that the heat applied in reducing the iodate of potash to iodide of potassium, might have either been insufficient for the purpose or have volatilized some of the latter salt, I recommenced the estimation of the amount of free iodine, or rather of that united to the oil, by a somewhat different process.

A known weight of the substance was introduced into a test tube with water and zinc filings, and the other end being

* This contains 9.58 grains of iodine.

drawn out at the spirit lamp, it was hermetically sealed so as effectually to prevent the volatilization of iodine. Agitation was now resorted to, and a gentle heat at the same time applied, which caused the separation of the oil, the iodine previously combined with it having entered into union with the zinc and formed with it a salt dissolved by the water. The tube was now broken, and its contents having been thrown upon a single filter previously deprived of all soluble matter, distilled water was poured on until the entire quantity of the iodide of zinc was carried through. The washings were concentrated, suffered to cool, and then treated with the equivalent quantity of nitrate of silver, and the resulting precipitate (iodide of silver) having been, as in the previous experiment, sparingly washed with cold water containing a little ammonia, was dried and weighed. From this the total quantity of iodine in the compound, both that combined with the potassium and with the oil, was collected. But the quantity in the former state having been already ascertained, the difference is the quantity of iodine associated with the oil.

In an experiment thus conducted 6.55 grains of the substance yielded of iodide of silver 4.52 grains, equivalent to 37.20 grains of iodine for 100 of the compound. Subtracting from this 9.58, the iodine of the iodide of potassium, we obtain, as the representative of the amount of this element associated with the oil, the number 27.62. Hence $\frac{28.66 + 27.62}{2}$

= 28.14 is the mean amount of the iodine in the latter state of combination as derivable from both experiments. But

$\frac{28.14}{9.58} = 2.93$, or $9.6 = 3$. We thus arrive at the conclusion

that for every atom of iodide of potassium in the substance under consideration there are three atoms of iodine in combination with the oil of cinnamon.

Before leaving this branch of the analysis, I may observe that the iodine of the oil may be directly obtained by decomposing the compound in a glass tube at a red heat in contact with lime, and acting upon the residue with water which dissolves the iodide of calcium, and along with it a little lime. The latter being separated in the usual manner by carbonic acid and boiling, the former may be precipitated by oxalate of ammonia, and the iodine estimated from the amount of carbonate of lime afforded by the oxalate when calcined at an obscure red heat.

The experiment made upon this plan did not give a very satisfactory result; and, when I considered the great dispro-

portion between the atomic weights of iodine and of lime I did not feel disposed to repeat the process.

The iodine may also be taken out of the compound by filings of iron as well as those of zinc, in the form of iodide of the metal; and, though the theoretical objection just stated to the process by lime is equally applicable to this method, a single experiment, whose particulars I subjoin, thus conducted led to a conclusion corresponding very closely with that already obtained.

8 grains of the compound gave 0.72 of peroxide of iron. But this amount of peroxide corresponds to 2.27 of iodine. Hence

$$8 : 2.27 :: 100 : 28.41 - \text{the}$$

percentage of iodine associated with the oil, and which exceeds the result, 28.14, obtained by the other methods by a quantity so small that it may be viewed as affording a corroboration of the correctness of the previous determination.

Having determined the iodide of potassium and the iodine in union with the oil, we can now state the composition of the compound, assuming the residue to be oil of cinnamon.

Iodide of potassium	12.55
Iodine	28.14
Oil of cinnamon	59.30
	<hr/>
	99.99

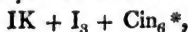
That it is the oil itself, and no oxidized or other modification of it, which exists in this compound, I have already assigned reasons for believing; and as, by the application of such heat as will fuse the compound, no water is set free, it becomes highly probable that the statement above made is a correct representation of its constitution. But the oil of cinnamon has been analysed, and through the researches of Dumas we are acquainted with its real composition, which he has shown to be represented by the formula $C_{18}H_8O_2$. If then the view numerically expressed above be the true one, the 59.30 parts of oil must correspond to some integer or at least simple number of atoms. And, reciprocally, if we find such to be the case, we shall be fortified in the conclusion which we have drawn.

With a view to this method of verification let the numbers which represent the iodide of potassium and iodine, and that which is supposed to represent the oil, be divided by their respective atomic weights, and let the quotients be reduced to others in the same ratio, and so that the iodide of potassium

may be represented by unity. When these arithmetical operations are performed we obtain the numbers in the second and third columns of the following table, the former being the quotients themselves, and the latter other numbers bearing to each other the same proportion.

	(1.)	(2.)	(3.)
Iodide of potassium ...	12·55	0·075	1·000
Iodine	28·14	0·223	2·973
Oil of cinnamon	59·30	0·442	5·893

The numbers, it will be seen, in the last column approximate so closely to the integers 1, 3, and 6, as to leave little doubt that the true empirical formula is



a conclusion which is strikingly confirmed by the following statement of the composition of our substance in 100 parts calculated upon this hypothesis.

Iodide of potassium	12·26
Iodine	28·08
Oil of cinnamon	59·66
	<hr/>
	100·00

To apply, however, to this conclusion the most decisive test, it remained to burn the substance with oxide of copper, and see whether the carbonic acid and water thus obtained would correspond with the amount of oil of cinnamon ascribed to the compound.

7·08 grains, Liebig's apparatus for potash being employed, yielded of carbonic acid 12·70 grains, and of water 2·60, equivalent to 3·513 carbon and 0·288 hydrogen. But, adopting for a moment the empirical formula already arrived at, the 7·08 grains of the substance would contain 4·223 of oil of cinnamon. If, therefore, from this we deduct the carbon and hydrogen, we obtain the oxygen, and find the constituents of the oil to be as follows:

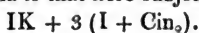
Carbon	3·513
Hydrogen.....	0·288
Oxygen	0·420

If these be divided by the atomic weights, and if also we substitute for the quotients numbers in the same ratio with them, that for carbon being assumed 18, we obtain the following:

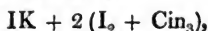
Carbon..	18·00
Hydrogen.....	8·82
Oxygen.....	1·60

* Cin is assumed as the symbol for the oil of cinnamon.

As the conjoint result, therefore, of our analysis and our hypothesis we find the formula for oil of cinnamon to be $C_{18}H_{8.82}O_{1.60}$. Now this is so close to the formula of Dumas, viz. $C_{18}H_8O_2$, particularly when we consider that owing to the fusibility of the compound, and the facility with which it is decomposed, heat could not be applied in drying the contents of the tube before the commencement of the combustion, and that consequently the hydrogen must have been too high and the oxygen too low,—considering this, I say, the accordance is so close as to leave no doubt that the empirical formula already given correctly represents the constitution of the compound submitted to analysis. It is scarcely necessary to say that the most probable rational formula is that here subjoined:



From the analysis which I first performed, and of which I gave a brief account in the Chemical Section at the Liverpool Meeting of the British Association for the Advancement of Science, the formula deduced was



which differs from the preceding merely in containing one more atom of iodine.

This compound appears interesting under many points of view. In the first place it is one of considerable complexity, is decomposed with an extreme facility, and is nevertheless perfectly definite in its composition, and even beautifully crystallized.

In the second place it is a kind of double salt, composed of two haloid salts, in one of which the oil performs the very unusual function of an electro-positive or basic metal,—a circumstance the more singular, as Dumas has shown that it unites also to the muriatic and nitric acids, forming with them binary compounds, the latter of which very readily crystallizes. The oil in fact thus appears to act the part of a metal as well as of an oxide.

Lastly, I may observe that the method by which our compound was first accidentally formed, and is still best made, presents an instance of incompatibility which had not been previously suspected, and will no doubt suggest to chemists experiments which will eventuate in the production of a series of similar substances. In reference, however, to this latter point I should add that Mr. Moore has applied to the other aromatic waters the very process which succeeds with cinnamon water, but without obtaining a trace of any new product. It is possible, however, that new results might be obtained by substituting other metals for the potassium, and replacing

the iodine by bromine or even chlorine; and I have indeed myself commenced some experiments with a view to this research.

XVII. *Researches upon the Composition of Coal.* By Mr. THOMAS RICHARDSON.*

[Illustrated by Plate III.]

WE are at present in possession of various analyses of coal, but at the time when these were made the method of analysis was too imperfect to enable any chemist to obtain accurate results. This fact, with the great and important use made of coal in manufactures, induced me to undertake the present researches. They have been conducted with every possible care and attention, and throughout I have been indebted to the kind instruction and advice of Professor Liebig. In the first part of the present memoir the various methods employed in determining the different constituents will be shortly described; and in the second part, the analyses of the various coals, &c.

I. METHODS EMPLOYED IN DETERMINING THE DIFFERENT CONSTITUENTS.

Hygrometric Moisture, &c.

The first object was to determine the amount of water which the coal contained, and whether this water was chemically combined, or merely hygrometrical. With this view the following experiments were made:

1. A certain quantity of coal, finely powdered, was dried at 100° C by means of Professor Liebig's apparatus, and the loss amounted to 1.23 per cent. 2. .854 grm.† coal, as finely pounded as the preceding, was dried in a chloride of zinc bath at the temperature of 185° C when it sustained a loss of .0105 or 1.229 per cent.

It may, therefore, be concluded that if coal contains water, it must exist in a state of the most intimate chemical combination.

Ashes.

The determination of the ashes was very simple. A weighed quantity of coal was heated to redness in a small platina crucible, till the whole of the carbon was oxidized, and the residue constituted the amount of ashes contained in the specimen.

* From the Transactions of the Natural History Society of Newcastle-upon-Tyne, vol. ii. p. 401.

† The measure of quantity used in these analyses is the French gramme; 1 gramme French = 15.433 grains English.

These ashes, treated with muriatic acid, afforded not the slightest perceptible trace of carbonic acid. When they were boiled with carbonate of soda, the clear filtered solution of the same, saturated by nitric acid, produced, not the smallest milkiness with a salt of barytes.

Carbon and Hydrogen.

The estimation of the carbon and hydrogen was partially made by means of oxide of copper, but generally with melted chromate of lead. The apparatus was perfectly the same as that employed by Professor Liebig, the only difference being in the use of the above salt. A more exact account of the employment of this substance will be found in another memoir.

Azote.

With respect to the method employed in the determination of the azote, it will be necessary to enter more minutely into particulars (though perhaps a digression from the proper subject of the present memoir,) to show that no exertions on my part have been wanting to obtain an exact result. Five or six analyses were made with the apparatus which Professor Liebig has already described for the absolute estimation of azote. The azote in all the analyses amounted to nearly 4 per cent. When it is remembered what a small quantity of sal ammoniac is obtained in every gas manufactory, this amount will at once appear much too great.

The method of determining the azote by means of its relation to the carbonic acid was then resorted to, but it was found impossible to measure the volume of the azote. I could only in this way guess at the true quantity of azote, and it appeared to be as 1 H : 100 CO₂.

At the suggestion of Professor Liebig, the following plan was then pursued:—The apparatus, as per diagram, (Plate III.) consisted of an ordinary tube of combustion *a*, about 20 inches long, and from .4 to .5 inch diameter, connected with a tube *b*, having 2 balls, and about 10 inches long and .4 inch in diameter. A small thermometer tube *c* was united to the other part of the apparatus by means of a piece of caoutchouc, and conducted the gas into a receiver *d*, which was partially filled with mercury. A part of the tube *c* remained always above the level of the mercury in the receiver *d*.

The tube of combustion *a* contained in the hermetically sealed end from 2 to 2½ inches of hydrate of lime, then one inch oxide of copper, afterwards the mixture of the substance

with oxide of copper, washings* of the same, a further quantity of oxide of copper, and lastly slips of metallic copper. The shaded part of the tube *b* was filled with hydrate of potash.

The whole apparatus being arranged, the receiver *d* was raised a little, and if the mercury retained its new level, the apparatus was considered air-tight. The air in the receiver *d* was now measured, the temperature and barometer being noted at the same. The combustion was conducted in the usual way, the water and carbonic acid were absorbed by the potash, while the azote forced the receiver to rise. When the combustion was finished, the hydrate of lime was heated slowly to redness, and the aqueous vapour thus formed drove out all carbonic acid into the tube of absorption. The apparatus was now allowed to cool, while the aqueous vapour condensed, and the increase of volume in the receiver denoted the quantity of azote in the substance submitted to analysis.

The precautions necessary to be taken are the following:—The mixture of the substance with the oxide of copper must be most intimately made, and the combustion proceeded with as slowly as possible; the pressure of the gas in the receiver must also be quite equable, otherwise the tube of combustion will be either increased or diminished in size, and consequently an incorrect result obtained; the heating of the hydrate of lime must be gradually performed; and care must be taken on the cooling of the apparatus that the condensed vapour which flows back be retained in the first ball of the tube of absorption, which is blown for this purpose.

By this method the following results were obtained:

·2768 grm. Uric acid were submitted to analysis.

27°5 Barometer at the time of the experiment.

12° Thermometer at the time of the experiment.

46·0 cc ... Air in the receiver before commencement.

120·0 cc ... Mixture of gas and air after cooling.

76·0 cc ... Azote.

76 cc azote reduced to 0° thermometer, and 28° barometer, give 70·4 cc which is equal to 32·24 per cent. From the analysis of Liebig, this acid contains 33·36 per cent., so that there is a loss of 1·12 per cent.

The analysis was repeated, but the loss was nearly the same.

* By washings I mean the oxide of copper employed in cleaning out the mortar wherein the mixture of the substance has been made.

- 3244 grm. Anhydrous amygdaline were submitted to analysis.
 27°7 Barometer at the time of experiment.
 12°6 Thermometer at the time of experiment.
 15·0 cc ... Air before commencement.
 20°5 cc ... Gas and air after cooling.

5·5 cc ... Azote.

5·5 cc Azote reduced to 0° thermometer and 28° barometer give 5·19 cc, equivalent to 2·03 per cent., which is, according to Liebig, 1 per cent. less than the true quantity.

From these experiments there appears to be a constant error of 1 per cent. This error was supposed at first to arise from a diminution of the oxygen in the air of the tube of combustion, destroyed in oxidizing the carbon and hydrogen. With the view to obviate this cause of failure, carbonate of copper was mixed with the oxide of copper, so that before the commencement of the decomposition of the organic body, the heat expelled the carbonic acid of the CO_2 , Cu O , and drove out all air in the tube of combustion.

With this modification—

- 3566 grm. Amygdaline were submitted to analysis.
 27°6 Barometer.
 11°4 Thermometer.
 14 cc ... Air in the receiver before commencing.
 20 cc ... { Gas and air at the close of the experiment after
 { cooling.

6 cc ... Azote.

6 cc Azote reduced to 0° thermometer and 28° barometer give 5·65 cc which equals 2·01 per cent. The loss here amounts again to 1 per cent.

- 2456 grm. Crystallized asparagin.
 27°4 ... Barometer.
 12°7 ... Thermometer.
 43 cc ... Air at the commencement in the receiver.
 81 cc ... Air and gas when finished.

38 cc ... Azote.

38 cc Azote reduced to 0° thermometer and 28° barometer give 35·49 cc which equals 18·32 per cent. The loss here amounts to ·64 per cent.

Oxide of copper having the property of absorbing carbonic acid from the air, which is expelled by the heat in the tube of combustion, and its place occupied by the mixture of azote and air in the end of the experiment, it was supposed that

this might be the cause of the error. Bichromate of potash and dichromate of lead were then successively employed instead of oxide of copper, but it was found impossible to avoid the formation of deutoxide of azote with these substances, in such quantities as to be reduced by the copper. Oxide of lead was also used, but the combustion was quite imperfect, and the quantity of ammonia formed, very great. Oxide of copper strongly heated, was at last employed, but the error remained the same.

The method remains thus at the present time, but I hope, by further investigation, to discover the cause of the error*. The error being thus confined to 1 per cent., two analyses of coal were made, in order to obtain some idea respecting the amount of the azote. These coals will afterwards be more minutely described.

- I. 283 grm. Coal from the neighbourhood of Edinburgh.
 27°7 Barometer.
 13°4 Thermometer.
 11·8 cc ... Air in the receiver.
 12·7 cc ... Gas and air after the experiment.

·9 cc ... Azote.

·9 cc Azote reduced to 0° thermometer and 28° barometer give ·84 cc which equals ·38 per cent.

II. 300 grm. Coal from Garesfield near Newcastle-on-Tyne, produced no gas, but there appeared to be a diminution of ·30 cc.

From the above experiments it clearly appears, that the coal cannot contain more than 2 per cent. of azote, but with the present means of analysis at our disposal it is impossible to determine the true amount.

II. ANALYSES OF THE COALS, &c.

The arrangement of the various coals, proposed by Dr. Thomson, has been for the present adopted in the following account. Two specimens of each of these varieties from different localities have been analysed. There are four varieties, viz., Splint, Cannel, Cherry, and Caking.

I. VARIETY—SPLINT COAL.

1. Specimen from Wylam Banks.

This coal is not at present worked; it is a thin bed very low down in the Newcastle coal series, and appears in this spot

* Since the above was first printed, it has occurred to me that the error arises from the absorption of the oxygen gas of the air in the apparatus, by the reduced oxide of copper, after the analysis is finished and during the cooling of the tube of combustion.

by the river Tyne cutting through it. Colour black; lustre glimmering; difficultly frangible; principal fracture imperfect conchoidal; cross fracture uneven and splintery; specific gravity 1·302.

The determination of the ashes, as already described, was

I. 1·234 grm. coal left as residue.....	·1715 grm.
II. ·0864 grm.....	·0122 grm.

1·3204

·1837

which amounts to 13·912 per cent.

Burnt in the usual way with oxide of copper :

I. ·270 grm. coal gave ·732 grm. CO ₂ and ·152 grm. H ₂ O
II. ·252 grm..... ·678 grm. CO ₂ and ·1385 grm. H ₂ O

Burnt with chromate of lead in the manner to be described:

III. ·3414 grm. coal gave ·927 grm. CO ₂ and ·1922 grm. H ₂ O
IV. ·3955 grm..... 1·0703 grm. CO ₂ and ·2176 grm. H ₂ O

These results give in 100 parts :

	I.	II.	III.	IV.
Carbon	74·961.....	74·381.....	75·071.....	74·878
Hydrogen.....	6·254.....	6·111.....	6·243.....	6·114
Azote and oxygen	4·873.....	5·596.....	4·774.....	5·096
Ashes	13·912.....	13·912.....	13·912.....	13·912
	<hr/> 100·000 <hr/>	<hr/> 100·000 <hr/>	<hr/> 100·000 <hr/>	<hr/> 100·000 <hr/>

The relation of the carbon and hydrogen is clearly as 1 : 1.

2. *Specimen from Glasgow.*

The splint coal occurs associated with cherry coal in the Glasgow coal field. The fifth bed is almost entirely constituted of this species. It is very highly esteemed for manufacturing and household purposes. Colour is black, with a little brown; lustre glimmering; difficultly frangible; fracture uneven and splintery; specific gravity 1·307.

Ashes determined in the usual way gave—

I. ·214 grm. coal left as residue ·0024 grm.
II. ·238 grm..... ·0027 grm.

·452

·0051

which amounts to 1·128 per cent.

Burnt with chromate of lead as usual—

I. ·2798 grm. coal gave ·838 grm. CO ₂ and ·1401 grm. H ₂ O
II. ·2596 grm. ·7818 grm. CO ₂ and ·1272 grm. H ₂ O
III. ·2378 grm. ·7115 grm. CO ₂ and ·1171 grm. H ₂ O

which yields in 100 parts.

	I.	II.	III.
Carbon	82·813.....	83·230.....	82·730
Hydrogen	5·562.....	5·442.....	5·469
Azote and oxygen	10·497.....	10·200.....	10·673
Ashes	1·128.....	1·128.....	1·128
	<hr/>	<hr/>	<hr/>
	100·000	100·000	100·000

The relation of the carbon to the hydrogen in this specimen is as 1·231 : 1·000 or 5 : 4.

III. VARIETY.—CANNEL COAL.

1. Specimen from Lancashire.

The locality of this specimen is Wigan, where it has for a long time been worked. From its capability of receiving a fine polish, it is made into toys, &c.

Its colour is greyish black; the lustre is highly glistening; fracture is large conchoidal. It is not so hard as the splint coal and is sectile. Specific gravity 1·319.

The ashes determined in the usual way were:

I. ·1706 grm. coal left as residue...	·0043 grm.
II. ·1825 grm.	·0047 grm.
<hr/>	<hr/>
·3531	·0090

which amounts to 2·548 per cent.

Burnt with chromate of lead in the usual way:

I. ·2937 grm. coal gave ·890 grm. CO ₂	
II. ·3178 grm. ·962 grm. CO ₂ and ·1624 grm. H ₂ O	
III. ·2819 grm. ·8545 grm. CO ₂ and ·1432 grm. H ₂ O	

or in 100 parts

	I.	II.	III.
Carbon	83·789 ...	83·698 ...	83·808
Hydrogen	5·677 ...	5·643
Azote and oxygen	8·077 ...	8·001
Ashes	2·548 ...	2·548 ...	2·548
	<hr/>	<hr/>	<hr/>
	100·000	100·000

The relation of the carbon to the hydrogen is in this coal as 1·207 : 1·000 or 6 : 5.

2. Specimen from Edinburgh.

This coal is called in Scotland *Parrot coal*, because its particles, when heated, fly off from one another with a crackling noise. It occurs in many of the series in the Edinburgh coal field. It splits easily, and throughout its substance se-

veral well-defined impressions of *Stigmaria ficoides* are found. Colour is black with gray; lustre approaches that of glistening; fracture, imperfect conchoidal; sectile and frangible; specific gravity, 1·318.

The determination of the ashes was as follows:

I. ·2007 grm. coal left as residue	·0295 grm.
II. ·1858 grm.	·0268 grm.
<hr/>	<hr/>
·3865	·0563

This amounts to 14·566 per cent.

Burnt with chromate of lead:

I. ·3022 grm. coal gave ·737 grm. CO ₂ and ·1468 grm. H ₂ O
II. ·294 grm. ·7205 grm. CO ₂ and ·1434 grm. H ₂ O

Expressed in 100 parts.	I.	II.
Carbon	67·434	67·760
Hydrogen	5·394	5·416
Azote and oxygen	12·606	12·258
Ashes	14·566	14·566
	<hr/>	<hr/>
	100·000	100·000

The relation of the carbon to the hydrogen is in this specimen as 1·020 : 1000 or 1 : 1. This differs from the cannel coal from Lancashire, but agrees with the splint coal of Wylam Banks, near Newcastle. Between the splint and cannel coals there is at all times much similarity, and this is countenanced again by the above results.

III. VARIETY.—CHERRY COAL.

1. *Specimen from Jarrow, near Newcastle.*

This species of coal occurs more or less in every coal field, often forming thin beds or layers in the midst of other coals. The specimen submitted to analysis was obtained from a thin seam passed through while sinking the shaft of the mine deeper. Colour, beautiful jet black; lustre, resinous, splendid; principal fracture, straight, uneven; cross fracture, conchoidal; not very hard and easily frangible; specific gravity, 1·266.

The determination of the ashes was as follows:

I. ·2567 grm. coal left as residue	·0045 grm.
II. ·3457 grm.	·0056 grm.
<hr/>	<hr/>
·6024	·0101

amounting to 1·676 per cent.

Burnt with chromate of lead.

I. 4164 grm. coal gave 12755 grm. CO_2 and 1895 grm. H_2O
 II. 3114 grm. 9573 grm. CO_2 and 1414 grm. H_2O
 expressed in 100 parts :

	I.		II.
Carbon	84.694	...	84.998
Hydrogen	5.054	...	5.043
Azote and oxygen	8.576	...	8.283
Ashes	1.676	...	1.676
	<hr/>		<hr/>
	100.000		100.000

The relation of the carbon to the hydrogen in this specimen is as 1.370 : 1.000 or 4.110 : 3.000.

2. Specimen from Glasgow.

The greater portion of the coal obtained from Glasgow consists of this species; it constitutes the chief part of the four uppermost beds.

Colour, jet black; lustre, not so splendent as that of Jarrow. In its other characters it is quite the same as the preceding specimen. Specific gravity 1.268.

The determination of the ashes was as follows :

I. 2410 grm. coal left as residue	0035 grm.
II. 1810 grm.	0025 grm.
<hr/>	<hr/>
4220	0060

which is equal to 1.421 per cent.

Burnt with chromate of lead :

I. 278 grm. coal gave 8148 grm. CO_2 and 1379 grm. H_2O
 II. 308 grm. 9073 grm. CO_2 and 1494 grm. H_2O
 III. 2721 grm. 7983 grm. CO_2 and 1338 grm. H_2O
 which produces in 100 parts :

	I.		II.		III.
Carbon	81.041	...	81.450	...	81.121
Hydrogen	5.509	...	5.387	...	5.461
Azote and oxygen	12.029	...	11.742	...	11.997
Ashes	1.421	...	1.421	...	1.421
	<hr/>		<hr/>		<hr/>
	100.000		100.000		100.000

The relation of the carbon to the hydrogen in this coal is as 1.216 : 1.000 or 6 : 5. This relation differs from that of the preceding specimen of this species.

IV. VARIETY—CAKING COAL.

1. Specimen from Garesfield, near Newcastle.

This specimen was obtained from one of the lowest seams
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in the Newcastle coal field. This coal is of a rich bituminous nature, caking or melting when heated; it is from this property that it receives its name.

Colour, black; lustre, shining, resinous; principal fracture, straight; cross fracture, uneven and cross-grained; the fragments have more or less a cubical shape; soft and very easily frangible; sectile. Specific gravity 1·280.

The estimation of the ashes was as follows:

I. ·2080 grm. coal left as residue.....	·0039 grm.
II. ·2800 grm.	·0029 grm.
<hr/>	<hr/>
·4880	·0068

which amounts to 1·393 per cent.

Burnt with chromate of lead:

I. ·2977 grm. coal gave ·9454 grm. CO₂ and ·1383 grm. H₂O
 II. ·3149 grm. 1·0035 grm. CO₂ and ·1509 grm. H₂O
 or in 100 parts:

	I.	II.
Carbon	87·809 ...	88·095
Hydrogen.....	5·159 ...	5·320
Azote and oxygen	5·639 ...	5·192
Ashes	1·393 ...	1·393
	<hr/>	<hr/>
	100·000	100·000.
	<hr/>	<hr/>

The relation between the carbon and hydrogen in this specimen is as 1·377 : 1·000 or 4 : 3.

2. *Specimen from South Hetton.*

This coal occurs in the county of Durham, and is worked through the magnesian limestone. It is regarded as one of the best coals. Its characters perfectly correspond with those of the preceding specimens. Specific gravity 1·274.

The ashes determined in the usual way were as follows:

I. ·2400 grm. coal left as residue ...	·0060 grm.
II. ·2604 grm.	·0066 grm.
<hr/>	<hr/>
·5004	·0126

which amounts to 2·519 per cent.

Burnt with chromate of lead:

I. ·2929 grm. coal gave ·8855 grm. CO₂ and ·1358 grm. H₂O
 II. ·2705 grm. ·8116 grm. CO₂ and ·1265 grm. H₂O
 expressed in 100 parts:

	I.	II.
Carbon	83·588 ...	82·960
Hydrogen	5·150 ...	5·193
Azote and oxygen	8·743 ...	9·328
Ashes	2·519 ...	2·519
	<hr/> 100·000 <hr/>	<hr/> 100·000 <hr/>

The relation in this specimen is the same as in the former, viz., 1·315C : 1·000H or 4 : 3.

For the sake of convenience and comparison, the following table contains the mean of the various analyses of each specimen.

TABLE I.

Species of Coal.	Locality.	Carbon.	Hydrogen.	Azote and Oxygen.	Ashes.
Splint	Wylam.....	74·823	6·180	5·085	13·912
.....	Glasgow	82·924	5·491	10·457	1·128
Cannel.....	Lancashire ...	83·753	5·660	8·039	2·548
.....	Edinburgh ...	67·597	5·405	12·432	14·566
Cherry	Newcastle ...	84·846	5·048	8·430	1·676
.....	Glasgow	81·204	5·452	11·923	1·421
Caking.....	Newcastle ...	87·952	5·239	5·416	1·393
.....	Durham	83·274	5·171	9·036	2·519

TABLE II.

Species of Coal.	Locality.	Quantity of Oxygen necessary to the perfect combustion of 100 parts of coal, subtracting the Oxygen contained in the coal.	Relative quantity of heat given out by the same weight of coal. Edinbro' = 100·00.	Relative quantity of heat given out by the same volume of coal. Edinbro' = 100·00.
Splint	Wylam...	240·1	110·34	108·99
.....	Glasgow	250·5	115·12	114·15
Cannel	Lancashire	256·4	117·83	117·91
.....	Edinburgh	217·6	100·00	100·00
Cherry	Newcastle	253·9	116·68	112·07
.....	Glasgow	244·0	112·12	107·78
Caking	Newcastle	266·7	122·56	119·03
.....	Durham...	250·2	114·98	111·31

The first table requires no explanation. The second table contains in the first column, that quantity of oxygen which 100 parts of the different coals abstract from the air for perfect combustion. This quantity of oxygen expresses the relative heating power of the different coals, in admitting that the quantity of heat evolved by a combustible substance is proportional to the quantity of oxygen which is consumed in its

perfect combustion. This relation, according to weight and volume, is given in the second and third columns.

For example, 100 volumes being taken, the Lancashire coal gives out more heat than the same volume of Edinburgh coal by a quantity expressed by 17·91: and 100 parts by weight being taken, the former coal surpasses the latter in the heat evolved by the quantity expressed by 17·83.

XVIII. *Arithmetical Analysis of mixed Salts of Potassium and Sodium.* By JOHN JOSEPH GRIFFIN, *Author of "Chemical Recreations."**

Analysis of Chlorides.

METHOD.

- a. **WEIGH** the chlorides.
- b. Dissolve the chlorides, precipitate with nitrate of silver, and weigh the chloride of silver.
- c. Multiply the weight of the mixed alkaline chlorides *a*, by 1·92404.
- d. Subtract the product of the multiplication *c*, from the weight of the chloride of silver *b*.
- e. Divide the residue of the subtraction *d*, by 0·52201.
- f. The product of the division *e*, is the weight of the chloride of sodium contained in the mixture *a*.
- g. The difference between the weight of the mixed chlorides *a*, and that of the chloride of sodium *f*, is the weight of the chloride of potassium.

Explanation.—The quantity of chloride of silver producible by chloride of sodium *above* the quantity producible by chloride of potassium, is 0·52201 for every unit of the given chloride of sodium. For,

1 of chloride of sodium produces of chloride of silver	} 2·44605
1 of chloride of potassium produces of chloride of silver	
	} 1·92404

The excess being = 0·52201

Consequently, in *any quantity* of chloride of silver produced by a mixture of chloride of sodium and chloride of potassium, we have

First, as much chloride of silver as is producible by a quantity of chloride of potassium of equal weight to the given mixture of chlorides; and

Secondly, as much more chloride of silver as is equal to

* Communicated by the Author: a paper on indirect chemical analysis, by Dr. G. Bird, will be found in vol. xii. p. 220.

the quantity producible by chloride of sodium above the quantity producible by chloride of potassium; this additional quantity of chloride of silver being equal to 0.52201 multiplied by every unit of chloride of sodium present in the mixture.

Wherefore,

In Operation c, we multiply the mixed chlorides by 1.92404 to find the quantity of chloride of silver producible by the given weight of chloride of potassium alone.

In Operation d, we subtract this quantity from that of the chloride of silver actually produced in experiment *b*, by the mixed alkaline chlorides; and thus determine the amount of the excess produced by the chloride of sodium.

In Operation e, we divide this excess by 0.52201 to ascertain the number of units, or the weight, of the chloride of sodium contained in the mixture.

Tables of Data.—1. To find the weight of the components and equivalents of any quantity of chloride of sodium, multiply it,

by 0.39656 for the sodium it contains.

0.60344 for the chlorine.

0.53289 for its equivalent of soda.

And by 2.44605 for the quantity of chloride of silver which it produces by precipitation.

2. To find the weight of the components and equivalents of any quantity of chloride of potassium, multiply it

by 0.52534 for the potassium it contains.

0.47466 for the chlorine.

0.63257 for its equivalent of potash.

And by 1.92404 for the quantity of chloride of silver which it produces by precipitation.

These tables explain the reason why a greater quantity of chloride of silver is produced by chloride of sodium than by chloride of potassium. The reason is that chloride of sodium contains 60 per cent. of chlorine, while chloride of potassium contains little beyond 47 per cent. I point out this fact, because it shows *the sort of differences upon which all indirect analyses must be founded.*

Example.—In the analysis of a mineral that contains both potassium and sodium we have proceeded so far as to have nothing to separate but these two metals, which are present in the condition of chlorides.

a. The mixture of chlorides weighs 5 grains.

b. It produces by precipitation 11.18623 grains of chloride of silver.

- c. The weight of the mixture *a*, 5 grains, multiplied by 1.92404 gives 9.62020.
 d. 9.62020 subtracted from 11.18623 gives 1.56603.
 e. 1.56603 divided by 0.52201 gives 3.
 f. The weight of the chloride of sodium in the mixture *a* is 3 grains.
 g. The weight of the chloride of potassium is 2 grains.
 The quantity of sodium equivalent to the chloride of sodium is 3×0.39656
 The quantity of soda is 3×0.53289
 The quantity of potassium equivalent to the chloride of potassium is 2×0.52534
 The quantity of potash is 2×0.63257

Analysis of Sulphates.

METHOD.

- a. Weigh the mixed sulphates.
 b. Dissolve the sulphates, precipitate with chloride of barium, and weigh the sulphate of barytes.
 c. Multiply the weight of the mixed alkaline sulphates *a*, by 1.33633.
 d. Subtract the product of the multiplication *c* from the weight of the sulphate of barytes *b*.
 e. Divide the residue of the subtraction *d* by 0.29814.
 f. The product of the division *e* is the weight of the sulphate of soda contained in the mixture *a*.
 g. The difference between the weight of the mixed sulphates *a*, and that of the sulphate of soda *f*, is the weight of the sulphate of potash.

Explanation.—The quantity of sulphate of barytes producible by sulphate of soda *above* the quantity producible by sulphate of potash, is 0.29814 for every unit of the given sulphate of soda. For

1 of sulphate of soda produces of sulphate of barytes 1.63447
 1 of sulphate of potash produces of sulphate of barytes 1.33633

The excess being = 0.29814

The further explanation of this method is the same as the explanation of the analysis of the alkaline chlorides.

Tables of Data.

1. To find the weight of the components and equivalents of any quantity of sulphate of soda, multiply it by
 0.326095 for the sodium it contains.
 0.673905 for the sulphur and oxygen.
 0.43819 for the soda.
 0.56181 for the sulphuric acid.

And by 1·68447 for the quantity of sulphate of barytes which it produces by precipitation.

2. To find the weight of the components and equivalents of any quantity of sulphate of potash, multiply it

by 0·449019 for the potassium it contains.

0·550981 for the sulphur and oxygen.

0·54067 for the potash.

0·45933 for the sulphuric acid.

And by 1·33633 for the quantity of sulphate of barytes which it produces by precipitation.

Example.—In the analysis of a sample of commercial alum, we have proceeded so far as to have nothing to separate but sulphate of potash from sulphate of soda, both of which alkalies have been detected by previous testing.

a. The mixture of sulphates weighs 7 grains.

b. It produces by precipitation 10·54687 grains of sulphate of barytes.

c. The weight of the mixture *a*, 7 grains, multiplied by 1·33633 gives 9·35431.

d. 9·35431 subtracted from 10·54687 gives 1·19256.

e. 1·19256 divided by 0·29814 gives 4.

f. The weight of the sulphate of soda in the mixture *a* is 4 grains.

g. The weight of the sulphate of potash is 3 grains.

The quantity of sodium equivalent to the sulphate of soda, is $4 \times 0·326095$

The equivalent of soda, is $4 \times 0·43819$

The quantity of potassium equivalent to the sulphate of potash, is $3 \times 0·449019$

The equivalent of potash, is $3 \times 0·54067$

The atomic weights employed in these calculations are those of Berzelius.

Glasgow, March 19, 1838.

XIX. *On some of the Phenomena and Laws of Action of Voltaic Electricity, and on the Construction of Voltaic Batteries, &c.* By CHRISTOPHER BINKS. *A second Communication, addressed to J. F. Daniell, Esq. F.R.S., &c., Professor of Chemistry in King's College, London. Part the Second.*

[Continued from Part i. p. 75.]

Section IV.

73. **T**HE ultimate object of the following experiments is to determine on the most advantageous construction of the voltaic battery; that is, such a construction as shall com-

mand its greatest effects, of any kind, with the least expenditure of materials.

The process by which this end is attempted to be reached, is by first determining the laws of action affecting the operations of *single* arrangements; and afterwards, when this is completed, extending the examination into the phenomena of *compound* ones.

74. These examinations are, in the first instance, restricted to the phenomena of arrangements in which sulphuric acid, diluted, is employed as the exciting agent. It is then sought to determine, by extending the inquiry into the less familiar operations of other kinds of arrangement, such, for example, as include the sulphate of copper as an element, whether the same results can be obtained under these as under the former conditions, or in what respects they differ; the chief object through all being the application of the principles, thus sought to be established, to the construction of the battery.

To determine the comparative amount of voltaic action induced in any single arrangement by acid solutions of different degrees of strength.

75. The plates of the voltaic couple here used were of an equal size, each presenting an entire surface of four square inches to the action of the acid mixture; they were separated from one another by a distance of half an inch: this, as well as every other attendant condition, being, of course, maintained exactly alike in every trial. The acid mixture was composed, in the first instance, of one part by measure of common sulphuric acid, and 100 parts of water; and afterwards of larger proportions of acid, as shown in the sub-joined table, in which the column of densities represents the actual strength of the mixture with the greater nicety. The amount of action in each case is estimated by the weight of zinc lost in a given equal time.

76. Table of the effects of acid mixtures of different degrees of strength. (No. 4.)

Parts by measure of Sulph. acid in 100 of water.	Specific gravity of the mixtures at Temp. 65° Fahr.	Quantity of Zinc in grains lost in a given time, or 10 minutes.	Effects compared with the first Result.
1	1·013	1·6	= 1
3	1·034	2·6	= 1·6
6	1·063	3·1	= 1·9
9	1·090	3·9	= 2·4
12	1·117	4·8	= 4·0
15	1·137	5·1	= 4·4
18	1·164	4·7	= 2·9
21	1·190	4·5	= 2·8
24	1·213	3·9	= 2·4

77. The comparative effects of these different acid solutions were then sought for under two distinct modifications of the experiments as just stated : first, when the elementary plates of the couple used were of a different size from those by which the results in the above table had been obtained ; and secondly, when the two plates of the couple were placed at a different distance from one another than that stated above ; but whilst every other attendant circumstance, in either case, was maintained precisely the same as at first.

78. For the former purpose a smaller couple was taken and immersed in the different acid mixtures successively, as before ; when the weight of zinc lost, in the same time, was of course less in the aggregate than when the larger couple was employed ; but the difference between the amount of loss occasioned by the different acid mixtures was precisely after the same rate as had been previously determined for the other couple, and as that rate is stated in the fourth column of the above table.

79. And when a couple was employed of the same size as the first (75.), but with its plates placed at a greater distance from one another than in either of the previous instances, in like manner to the last, the action in the aggregate was reduced by reason of the greater distance, but the comparative rate at which each acid mixture acted upon this couple was precisely the same as had already been found in the two previous instances.

80. It appears, therefore, that the comparative effects of these different acid solutions are the same whatever may be the size of the voltaic couple, or whatever may be the distance between its elementary plates ; and the above table, therefore, represents the comparative rate in which dilute sulphuric acid of different degrees of strength acts upon any voltaic arrangement.

81. A review of this table shows, 1st, that the greatest amount of action induced in any arrangement by dilute sulphuric acid takes place when the mixture is in the proportion of about 15 parts by measure of ordinary acid, and 100 of water ; or of the average specific gravity of 1.140 : 2ndly, that the rate of increase of action is neither the same as the rate of increased proportions of the acid, nor of the specific gravity of the mixture, but occurs in some other simple rate, bearing however no very obvious relation to any apparent attendant circumstances.

82. The acid mixtures which will subsequently be employed in these experiments are the first four of the above table.

Section V.

To determine the comparative amount of action in any single voltaic arrangement when its plates are placed at different distances from one another.

83. The two plates of a voltaic couple may be either of an equal size, or unequal, and the difference in size which may exist between the two is unlimited.

The couple itself, considered as a couple, may either be large or small also, to an unlimited extent.

The distance between the two plates of any couple, whatever its size, or whatever the relative proportions of the plates, may also be varied without limitation.

The acid mixtures also, in which any voltaic couple is made to operate, may be of any required degree of strength.

84. The immediate object in hand is to determine the effects of distance; but these must be sought for under every possible condition of the arrangements as regards the size of the couple used, the relative proportions of its plates, and the strength of the exciting acid.

To determine the law of distance when the two elementary plates are of an equal size.

85. (a). A voltaic couple having on each plate an entire surface of 6 square inches, had its plates placed successively at the distance from one another of $\frac{1}{4}$ of an inch, 4 inches, and 24, and the quantity of hydrogen by measure, yielded in a given time at each of these positions, was respectively equal to

84. 58 and 39 $\left\{ \begin{array}{l} \text{in 50ths of a} \\ \text{cubic inch.} \end{array} \right.$

86. (b). Another couple, exactly one half the size of the last, yielded under precisely the same conditions the respective measures of

46. 31 and 21 $\left\{ \begin{array}{l} \text{in 50ths of a} \\ \text{cubic inch.} \end{array} \right.$

87. (c.) Another couple, one fourth the size of the first one, and placed under the same circumstances, yielded the numbers

26. 18 and 12 $\left\{ \begin{array}{l} \text{in like measures of} \\ \text{hydrogen in the same time.} \end{array} \right.$

88. These experiments determine that whatever may be the size of the couple itself, its elementary plates being *equal*, the influence of distance upon its action is the same.

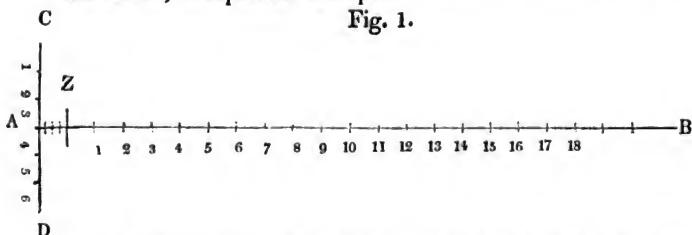
89. The total amount of action under condition *a*, at whatever position it is taken, greatly exceeds the total amount of action under *b* or *c*; but the ratio of the difference between

the amount of action obtained at any of the three different positions in *a* is the same as the ratio found for these positions respectively in *b* and *c*.

90. $12 : 26$ (in *c*) $:: 21 : 45.5$ (in *b*) being as near an approach to 46, the real number, as could be looked for in actual experiment; and the same ratios are maintained with no material alteration throughout.

91. These preliminary trials show, therefore, that I may select any-sized couple which may appear most convenient for the following more extended experiments upon the effects of distance; and that the results obtained by the couple now to be used will be true equally for this one condition and for every other as regards the dimensions of the couple employed.

92. To facilitate references to positions and numbers in the following experiments, let the mass of liquid in which they are conducted be represented by the following diagram, in which the horizontal line is supposed to pass through the centre of the mass contained in the graduated trough already described (36. p. 64), and the vertical line, also passing through the centre, to represent its depth.



93. The position of the fixed zinc plate is at *Z*, whence the graduation commences; the first division being a quarter of an inch from the zinc, and the amount of voltaic action obtained at this first position is used throughout as the standard of comparison of the effects of distance. The plates used in these experiments, whether of zinc or copper, are each one inch square, and only that surface of the zinc plate which is opposite to the copper presents a clear amalgamated surface; the contrary surface, as well as the connecting wire, being well covered with wax to preclude the contact of the acid, and to restrict its action to the clear amalgamated surface alone. But both surfaces of the copper are clear, and consequently brought into operation.

94. The experiments are first gone through with an acid mixture composed of 1 part by measure of sulphuric acid, and 100 of water, its specific gravity being 1.013; and are after-

wards repeated with other acid mixtures of the strength stated in the subjoined table. The copper plate being moveable at pleasure, is fixed, first, at the nearest position to the zinc, and afterwards in succession at each succeeding position marked upon the horizontal line. All other particulars affecting such experiments have already been sufficiently adverted to throughout section 3rd, and the observances there stated as necessary being fully discussed in that section, will in no instance be restated or again referred to in the course of the details that now follow. The amount of voltaic action obtained at each position, is estimated by the length of time in seconds required for the production of $\frac{1}{10}$ th of a cubic inch of the hydrogen which is evolved from the copper plate.

95. Table showing the effects of distance, (No. 5.), in which the comparative amount of voltaic action is estimated by the length of time in seconds required for the production of one measure of gas.

Distance, in inches, of the two plates from one another.	Proportions by measure of the acid and water, and specific gravity of the mixtures. Temperature 55° Fahrenheit.			
	1 part acid, 100 water. Sp. gr. 1.013.	3 parts acid, 100 water. Sp. gr. 1.034.	6 parts acid, 100 water. Sp. gr. 1.063.	9 parts acid, 100 water. Sp. gr. 1.090.
	Time. 180"	Time. 85"	Time. 60"	Time. 45"
$\frac{1}{4}$	245	135	110	95
1	370	170	120	120
2	375	155*	125	125
4	345*	170	120.	125
6	400	170	130	125
8	460	170	130	140
10	485	185	130	145
12	515	200	145	145
14	530	205	145	145
16	545	215	165	130.
18	590	220	165	145
20	640	230	165	145
22	655	235	165	145
24	660	265	190	145
26	680	295	190	145
28	690	290	190	170
30	790	300	190	200
32	805	310	220	210
34	825	360	220	170
36	885	365	200	170
38	900	370	230	170
40	910	370	240	170
42	920	370	240	170
44				

96. The results which are registered in this table were

those which were obtained the latest of all from the experiments gone through to determine the law of distance. No reliance was placed upon the first attempts made to determine this question, nor upon any, till by innumerable repetitions of experiments, and a perfect familiarity with the precautions necessary to be observed in their course, I had become assured that the whole were accurately performed, and that by the regulations adopted every possible or probable source of error likely to arise from the method here employed was precluded. The above results may therefore be considered to show correctly the peculiar phenomena attendant upon voltaic action, in the kind of arrangement here brought into action.

97. The first consideration which naturally follows a review of the above table, is that of the singular difference in the effects of distance upon voltaic action; as those effects are determined by this method of experimenting, and those already deduced by the indications of the magnetic needle. Referring to the nearest authority at hand, I find that the law of distance as determined by the needle is as follows:

98. "The deflection produced by a pair of plates in an acid solution of uniform strength varies inversely as the square root of the distance between them, a law previously established by Cumming. Thus, if a plate of zinc be placed successively at 1, 4 and 9 inches from a plate of copper, the deflecting powers will be in the ratio of 3, 2, and 1; that is, only twice as great at 1 inch as at 4, and only three times as great at 1 inch as at 9*."

99. As the magnetic galvanometer, of whatever construction, is employed as a measurer only of comparative quantities of electricity, and not of the absolute quantity evolved by any arrangement, it is in this relation merely that the indications of that instrument are now brought into comparison with those afforded by the method here used for the same purpose. When the quantities of electricity evolved at the several distances are estimated by the indications of the needle (that is, by their power to deflect the needle, in opposition to the power of the earth's magnetism, or to any other power substituted for it, as in the torsion galvanometer) then those quantities differ from one another by the rate just stated; but when estimated directly by the quantities of matter expended in producing them (on which principle the plan now used is founded), then they differ from one another at these several distances by a rate totally different from that de-

terminated by the indications of the needle; thus placing the results of these two methods of estimating such effects completely at variance.

100. On referring to the above table it will be seen that no such law, nor any making the most remote approach to it, can be deduced from the results obtained by the present method of testing such phenomena.

101. The difference between the degrees of voltaic action obtained at the first position, and at the last, in the first column of this table, amounts only to the difference between 5 and 1, nearly; the voltaic action yielded at the first or nearest position being about five times greater than that yielded at the most remote. The distance from one another of the two plates at the first position is $\frac{1}{4}$ of an inch, and at the last is 44 inches, and these distances comparatively are as 1 and 44×4 or 176; the distance of the two plates from one another being 176 times greater at the last than at the first position.

102. Now had the law of distance found by the method here employed been the same as that determined by the magnetic galvanometer, we should have had the amount of action at the first position greater than that at the last, by the square root of the difference in their distances, or by the square root of 176; but in actual experiment it is only 5 times greater instead of about 13 times, in round numbers.

103. This discrepancy in the results obtained by these two methods, in neither of which there is reason to doubt the correctness of the observations, leads to the suspicion that either the one or the other of them is an incorrect measure of comparative quantities of voltaic electricity, or that both are unfit to be applied to that purpose; or at least are imperfect in their indications; a conjecture which has given rise to the inquiry contained in the second part of this paper as already mentioned (13.). A comparison between these two methods will then be instituted, when it will be shown that there is reason to conclude that the needle does not take cognizance of the *whole* effects resulting from voltaic action, but only of a part of its attendant phenomena; and when also an attempt will be made, experimentally, to distinguish between the two kinds of action induced by such voltaic arrangements, of which each method is suspected to be respectively the measurer. But not further to anticipate that inquiry at this moment, I proceed to examine some other results afforded by this table.

104. The difference between the amount of action obtained at the first and last positions has been seen to be as 5 to 1

with the acid mixture of the first strength; but it will be observed that this difference is progressively less as the mixture increases in strength. With the second mixture the difference is about $4\frac{1}{4}$ to 1; with the third it is 4 to 1; and with the fourth it is $3\frac{3}{4}$ to one nearly—showing that the stronger the acid, or the greater its density, or the greater the activity of the generating agents, the less marked are the effects, in decreased action, caused by the difference in the relative distance of the two plates.

105. Again, it is observable that the decrease in action occasioned by increased distance does not proceed at a rate corresponding to that increase in distance, as the copper plate is removed successively to each position from end to end. The greatest effects of this removal through an equal distance occur in the first two or three positions in every column; after which the effects of removal are much less marked throughout.

106. At those positions distinguished by an asterisk, there occurs a slight increase of action compared with the amount immediately preceding it, instead of a decrease as might have been expected. A similar anomaly, though not to the same extent, presents itself at several successive positions throughout every column, where the voltaic action appears to alternate, or to be suspended between increase and decrease in its amount, compared with the amounts obtained at the adjacent positions. Taking the first column by way of example, it will be seen that scarcely in any two instances does the removal of the plate through an equal distance produce an equal effect in the resulting action. The difference in effect caused by removing the plate from 8 inches to 10 amounts to 60'', whilst that yielded by the change from 10 to 12 amounts only to 25''. The following table, derived from the first column above, will serve to show the nature and extent of this alternation more clearly. The first line contains the successive distances to which the plate is removed; the second, the difference between the amount of decrease obtained at each position and the one preceding it.

Table (No. 6.)

107. (Distance of the two plates from one another, by successive equal steps of two inches each).

10, 12, 14, 16, 18, 20, 22, 24, 26, 28, 30, 32, 34, 36, 38, 40,
42, 44.

(Amount of decrease resulting from increased distance, obtained at each position, compared with the decrease at that immediately preceding).

60'', 25'', 35'', 15'', 15'', 45'', 50'', 15'', 5'', 20', 10'', 100'', 15'',
20'', 60'', 15'', 10'', 10''.

108. This peculiar result is equally, or even more, obvious in the remaining columns of the former table, No. 5; and it is observable that the particular positions at which the alternation occurs are different in each. In the latter columns, in which (by reason of the greater activity of the action, the time over which each experiment extends is progressively shorter), the difference in many instances is so little, if any, as to be scarcely discernible; and they consequently present at several positions a series of numbers equal in amount and following each other in succession.

109. It was first attempted to arrive at the law of distance, by the results afforded at fewer and more remote positions of the plates, than those given in the above tables; for instance, the positions were taken at $\frac{1}{4}$ of an inch, 1 inch, 4, 12 and 24 inches; and the amounts of action obtained at these presented a very regular decrease corresponding to the increase in distance. But some occasions arose, in which it became necessary to test the action of the plates at other positions intermediate to those already tried, when the results obtained were occasionally so greatly at variance with any anticipated by the former trials, that it became necessary to carry the copper plates through shorter successive positions from end to end, to determine whether or not those which had thus been accidentally detected were merely the result of accident, or of some error in the method of observation, or were in fact part of the general phænomena attendant upon the voltaic action, as it takes place in the kind of arrangements here brought into operation. Hence the long columns of observations contained in the former table No. 5, in the place of which it might have been presumed beforehand, that a very few experiments comparatively would have been equally competent to decide the point in question, viz. the effects of distance.

110. I should have continued, as at first, to attribute these unlooked for results to some accidental circumstance, had not their invariable recurrence under like conditions of experiment, shown that they had some connexion, whatever that might be, with the general phænomena attending these operations, and were due neither to inaccuracy nor to accident.

111. It might be suspected, among other attempts to account for it, that the plan here resorted to (see 64) of changing the zinc plate after every two or three immersions, might have some share in producing this apparent alternation in effect. But the same results follow precisely if one plate only

be used throughout. Or, again, that it might arise in part from the practice of determining the measure of hydrogen, sometimes by a division near the top of the long tubular meter here used, and sometimes at the bottom of it; under which different circumstances, the volume of an equal measure of gas would be a little different by reason of the varying strain upon its elasticity. But the trifling variation that might have arisen from this cause, was also avoided by invariably refilling the meter after each single experiment, so that the $\frac{1}{10}$ th of a cubic inch of gas was always under a uniform pressure. But in short, after the utmost attention to the subject, I could discover no peculiarity attending this particular method of experimenting to which this effect could be attributed, with the most remote appearance of probability.

112. Such alternation then must be considered as a part of the general phænomena attending operations of this kind, however unexpected or inexplicable it may be in the present state of our acquaintance with the subject generally.

113. I was unwilling in the first instance to register the results as they stand above, expecting that every succeeding repetition of experiment would show a greater regularity in the operation of the arrangements; or such a regularity as, by preconceived notions, derived chiefly from the law above quoted (98), I had been led to anticipate. It is obvious however, that the operations brought into exercise by voltaic arrangements of this description at least, are of a mixed and complicated kind, influenced in some parts by causes as yet undetected, and are certainly such as cannot be fully included in any law similar to that just alluded to. The removing of the plates further from one another does not affect their action, merely by decreasing its amount, much less does that decrease occur in the ratio stated in that law.

114. Perceiving at this stage of the inquiry no satisfactory or probable way of accounting for this peculiar result, I proceed on to the further experiments, in which it will be seen that other indications of the same phænomenon can be detected in every direction, whether or not the results finally obtained may be considered in every respect as contributing satisfactorily to its explanation.

[To be continued.]

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xii. p. 433.]

April 26, **A** Paper was read, entitled, "An Account of a line of Levels carried across Northern Syria, from the Mediterranean Sea to the River Euphrates." By William Taylor Thomson, Esq., with Geological and Botanical Notes, by William Ainsworth, Esq. Communicated by Captain Beaufort, R.N., F.R.S., &c.

The operation of carrying a line of levels across Northern Syria, from the Mediterranean sea to the river Euphrates, was undertaken by Colonel Chesney, at the time he commanded the expedition sent to that river in the year 1835, chiefly with a view to determine the capabilities of the intervening country for the establishment of communications by roads, railways, or canals; but it was expected also that the examination would afford information of much historical and geographical interest. It was commenced in August of the same year, by Lieutenant Murphy and Mr. Thomson, assisted by Sergeant Lyne, R.E., Gunner Waddell, and some Maltese: but most of the party being disabled by sickness, and their numbers reduced by deaths and removals, the levelling was at length conducted principally by Mr. Thomson, with the assistance, in the latter part of the work, of Mr. Elliott, commonly called Dervish Ali. The result of this great labour was to determine the bed of the Euphrates to be 628 feet above the level of the Mediterranean.

The whole of the district over which the line of levels was carried naturally divides itself into four regions, each of which is characterized by its relative elevation, its peculiar geological structure, its vegetation, and the manners and habits of its population.

The first region, commencing from the Euphrates, comprises the country of the upper chalk and conide limestones, which averages an elevation of 1300 feet, and is but slightly undulated. The soil is light, somewhat stony, and of no great depth, and is highly productive in crops of corn and cotton. These uplands are inhabited by stationary Turcomans and Arabs, who are a mixed race of Fellahs. The large plains of this region are studded over in every direction with numerous mounds, of a more or less circular form, called by the Arabs *Tels*, and by the Turcomans *Heuks*, the origin of which appears to be partly natural and partly artificial. A village is found at the foot of almost every one of these monticules.

The second region comprises the country of ostracite limestone and feldspath pyroxenic rocks, in the valley of Ghuidaries and the Aphrean, having a mean elevation of 450 feet. This district is extremely fertile, for the most part cultivated, and inhabited by agricultural Kurds.

The third region is the lacustrine plain of Umk, elevated about 305 feet above the Mediterranean, and covered, for the most part, with the gramineous plants which feed the flocks of the pastoral and nomadic Turcomans.

The fourth region, formed by the valley of Antioch, is rocky, irregular, and varying from elevations of 220 to 440 feet. It comprises also the alluvial plain of the Orontes, which gradually sinks to the level of the Mediterranean. This latter district is covered with shrubs, which are chiefly evergreens; and inhabited by a few families of Syrians, who, in these picturesque solitudes, chiefly follow mysterious rites, presenting a mixture of Mahomedanism and Christianity.

It appears, from the examination of this line of country, that there here exist two distinct regions, the one low and already furnished with the means of water transport; and the other elevated, where the waters, which are lost in the valley of Aleppo, might be turned with facility into an artificial channel. Both regions are remarkably level, and present, when separately viewed, very few difficulties to be overcome for the construction of artificial roads.

May 3.—A paper was read, entitled, "Supplementary Note to the Eleventh Series of Experimental Researches in Electricity." By Michael Faraday, Esq., D.C.L., F.R.S., &c.*

The author describes, in this supplementary note, experiments made with the view of determining the specific inductive capacities of dielectrics, by means of an apparatus of the following form. Three circular brass plates were mounted, side by side, on insulated pillars; the middle one was fixed, but the two outer plates were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires, connecting each of the leaves respectively with the adjacent outer plate. The amount of disturbance in the electric equilibrium of the outer plates produced by interposing a plate of the dielectric substance to be tried, after charging the middle plate, was taken as a measure of the specific inductive capacity of that dielectric. By varying the size and distances of the plates, and also the distance of the gold leaves from one another, new conditions are supplied for the more exact determination of the relative inductive powers of dielectrics of every description; and by sufficiently reducing the dimensions of the instrument, it may be rendered applicable to comparatively small masses of dielectrics, such as crystals, and even diamonds. An instrument capable of such universal application the author proposes to designate by the name of *Differential Inductometer*.

Also read, a Letter addressed to P. M. Roget, M.D., Secretary to the Royal Society, by James Ivory, Esq., F.R.S., accompanying a paper on Astronomical Refractions. Communicated by Dr. Roget.

The author adverts in this letter to the attempts made by Newton to solve the problem of atmospherical refractions, which were baffled by the experience that the observed quantities fall far short of the theoretical deductions; whence he justly inferred that some new cause must be sought for capable of effecting that change in

* An abstract of Prof. Faraday's Eleventh Series of Researches was given in Lond. and Edinb. Phil. Mag. vol. xii. p. 358.—EDIT.

the density of the lower part of the atmosphere which is required for reconciling theory with observation. It becomes necessary, in particular, to investigate the law according to which the temperature diminishes as the height increases. The initial value of the rate of diminution has to be determined by experiment; and the introduction of this new element into the equation of an atmosphere in equilibrium must be an approach to the true solution of the problem of the refractions, and is indispensable if arbitrary assumptions are to be avoided. The author proceeds to notice Laplace's solution, which, though highly ingenious, is nevertheless hypothetical; and he adverts to the want of precision exhibited in Biot's dissertation on the influence which the presence of aqueous vapour in the air has on the refractions: but refers to the paper which accompanies his letter for the further explanation of his views on this subject.

A paper was also in part read, entitled, "On the Theory of the Astronomical Refractions," by James Ivory, Esq., K.H., M.A., F.R.S., &c.

May 10.—The reading of Mr. Ivory's paper "On the Theory of the Astronomical Refractions," was resumed.

May 24.—The reading of the paper by Mr. Ivory, "On the Theory of the Astronomical Refractions," was concluded.

In this communication, the author, after stating that the mean refractions are the object of investigation, and fully defining what he understands by this term, gives an historical review of what has been done up to the present time on this very important subject. Having stated that the foundation of the theory of astronomical refractions was laid by Dominique Cassini, he deduces on Cassini's hypothesis (that of an homogeneous atmosphere) a formula for the refraction, which agrees exactly with that of La Place, employed in computing the first part of the table of mean refractions, published by the French Board of Longitude.

The labours of our immortal countryman Newton, in this vast field of inquiry, are next reviewed. As the density of the atmosphere in ascending decreases gradually, the path described by a ray from a star, in its passage through the atmosphere, is not a straight line, as it would be on Cassini's hypothesis, but is a curve more and more inflected towards the earth's centre. In the *Principia* there is found whatever is necessary for determining the nature of this curve, and, consequently, for solving the problem of the astronomical refractions, which consists in ascertaining the difference between the direction of light when it enters the atmosphere, and its ultimate direction when it arrives at the earth's surface.

On the principles established in the second section of the *Principia*, the author deduces equations requisite for the solution of the problem of astronomical refractions, and remarks that these equations are perfectly general, and will apply in any constitution of the atmosphere that may be adopted. In this investigation, in preference to employing functions with peculiar properties to express the molecular action, the manner in which the forces act has been considered. When the light, in passing through the atmosphere, arrives at

a surface of increased density, it receives an impulse which may be considered as instantaneous; and this impulse being distributed over the breadth of a stratum of uniform density, ascertains the centripetal force tending to the earth's centre, by the action of which the trajectory is described.

It appears, that Newton himself was the first to apply this new method to the problem of the astronomical refractions. In his first attempt he assumes that the densities decrease in ascending, in the same proportion as the distances from the earth's centre increase. On this supposition the author investigates a formula, which M. Biot has also obtained, and which is equivalent to the construction communicated by Newton to Flamsteed. On this basis a table was computed and communicated to Flamsteed; but Newton subsequently informed Flamsteed that he did not intend to publish it, in consequence of a serious objection to the supposed scale of densities. Adopting the principles in the twenty-second proposition of the second book of his *Principia*, Newton, it appears, succeeded at length in computing a second table of refractions, which he likewise communicated to Flamsteed, and which, there is every reason to think, is the same which he gave to Halley, and which was inserted by that astronomer in the *Philosophical Transactions* for 1721. As the determining whether the two tables are identical is a question of much interest, the author enters very fully into it, and, from the results of elaborate calculations, concludes that Halley's table is no other than the one which Newton calculated on the supposition that the densities in the atmosphere are proportional to the pressures. He remarks that, as far as the mathematics are concerned, the problem of the astronomical refractions was fully mastered by Newton.

After referring to the labours of Brook Taylor, Kramp, and Thomas Simpson, the author again adverts to Newton's views, remarking that, in assigning the rarefaction of the lower region of the atmosphere by heat as the cause why the calculated refractions near the horizon so much exceeded the observed, as was found to be the case, Newton had assigned the true cause; but that he had no clear conception of the manner in which the density in the lower region is altered by the agency of heat; and he considers that nearly the same ignorance in that respect still prevails.

The two atmospheres, with densities decreasing in arithmetical and geometrical progression, which, it now appears, were imagined by Newton, and which have been discussed by Thomas Simpson and other geometers, are found, when the same elements are employed, to bring out horizontal refractions on opposite sides of the observed quantities. La Place conjectured that an intermediate atmosphere which should partake of the nature of both, and should agree with observation in the horizontal refraction, would approach nearly to the true atmosphere. If recourse be had to the algebraical expressions of La Place, it will be found that the atmosphere he proposes is one of which the density is the product of two terms, the one taken from an arithmetical, the other from a geometrical series; the effect of which combination is to introduce a supernumerary con-

stant, by means of which the horizontal refraction is made to agree with the true quantity. The author considers, with Dr. Brinkley, that the French table, founded on La Place's investigation, is only a little less empirical than the other tables, and that the hypothesis of La Place does not appear to possess any superiority over other supposed constitutions of the atmosphere in leading to a better and less exceptionable theory.

After eulogizing Bessel's tables of mean refractions, published in his *Tabulæ Regiomontanæ*, the author refers to his own paper in the Philosophical Transactions for 1823*. In this paper the refractions are deduced entirely from the very simple formula,—

$$\frac{1 + \beta r'}{1 + \beta r''} = 1 - f(1 - c^{-u})$$

in which β stands for the dilatation of air or gas by heat, r' and r'' for the temperature at the earth's surface, and at any height above it, and c^{-u} for the density of the air at that height in parts of its density at the surface. If this formula be verified at the earth's surface in any invariable atmosphere, by giving a proper value to the constant f , it will still hold, at least with a very small deviation from exactness, at a great elevation; and this is immediately shown.

This manner of arriving at the constitution of the atmosphere is contrasted with the procedure of M. Biot of transforming an algebraical formula, for the express purpose of bringing out a given result. As the problem in the *Mécanique Céleste* is solved by means of an interpolated atmosphere between two others; as in Mr. Ivory's paper of 1823, there is no allusion to such an atmosphere; and as the table in that paper is essentially different from all the tables computed by other methods, he contends that all these must be sufficient to stamp an appropriate character on his solution of the problem. But if ingenuity could trace some relation, in respect of the algebraic expression, between the paper of 1823 and La Place's calculations, he considers that it is not difficult to find, between the same paper and the view of the problem taken by the author of the *Principia* in 1696, an analogy much more simple and striking. Newton having solved the problem, on the supposition that the density of the air is produced solely by pressure, and having found that the refractions thus obtained greatly exceeded the observed quantities near the horizon, inferred, in the true spirit of research, that there must be some cause not taken into account, such as the agency of heat, which should produce, in the lower part of the atmosphere, the proper degree of rarefaction necessary to reconcile the theoretical with the observed refractions. The author's sole intention, in introducing the quantity f in his formula, is to cause the heat at the earth's surface to decrease in ascending, at the same rate that actually obtains in nature, not before noticed by any geometer, but which evidently has the effect of supplying the desideratum of Newton.

* Mr. Ivory gave an account of the theory of refractions enunciated in his paper of 1823, in the Phil. Mag. First Series, vol. lxiii. p. 420.—Eerr.

The author considers, that the comparison of the table in the paper of 1823, with the best observations that could be procured at the time of publication, was satisfactory; and after the publication of the *Tabulæ Regiomontanæ*, he found that the table agreed with Bessel's observed refractions to the distance of 88° from the zenith, with such small discrepancies as may be supposed to exist in the observations themselves.

The paper in the Philosophical Transactions for 1823, however, takes into account only the rate at which the densities, in a mean atmosphere, vary at the surface of the earth; but, in the present communication, the author proposes to effect the complete solution of the problem, by estimating the effect of all the quantities on which the density at any height depends. For this purpose, he finds it necessary to employ functions of a particular kind; and then gives a formula, one part of which consists of a series of these functions, for the complete expression of the temperature of an atmosphere in equilibrium; the intention of assuming this formula being to express the temperature in terms of such a form as will produce, in the refraction, independent parts that decrease rapidly. By this means he proceeds in the analytical investigation of the problem in its more comprehensive form, and deduces two equations on which its solution depends.

The first of these contains the law according to which the heat decreases as the height above the earth's surface increases; and the second determines the perpendicular ascent, when the difference of the pressures and of the temperatures at its upper and lower extremity have been found. If the latter, with a slight transformation, be multiplied by the proper factor, representing the variable force of gravity in different latitudes, it becomes identical with the usual barometric formula, all its minutest corrections included; and it has this advantage; that, whereas the usual formula is investigated on the arbitrary assumption, that the temperature is constant at all the points of an elevation, and equal to the mean of the temperatures at the two extremities, this formula is strictly deduced from the general properties of an atmosphere in equilibrium.

Having determined, from experimental results, the values of certain constants in these formulæ,—first, in an atmosphere of dry air, and, secondly, in an atmosphere of air mixed with aqueous vapour, the author remarks, that the analytical theory agrees in every respect with the real properties of the atmosphere, as far as these have been ascertained.

The object of Mr. Ivory's further investigation is to show, that the same theory represents the astronomical refractions with a fidelity that can be deemed imperfect only as far as the values of particular constants, which can only be determined by experiment, are liable to the charge of inaccuracy. He therefore proceeds to determine, from the formulæ previously deduced, the refraction of a star in terms of its apparent zenith distance. For this purpose, the differential equations are transformed by the introduction of new symbols; the limits of certain terms are determined previously to

their being neglected; and the equation is finally reduced to a form, in which the remaining operations consist in investigating the integrals of four expressions, and in subsequently assigning their numerical values. Great skill is displayed in conducting these intricate investigations; and after going through the most laborious calculations and computations, the author exhibits a table of theoretical refractions, deduced solely from the phenomena of the atmosphere, for zenith distances, extending from 10° to $89\frac{1}{2}^\circ$. These refractions are compared with those in Bessel's table, in the *Tabulæ Regiomontanæ*, and also with those in the table in the *Connaissance des Temps*. From this comparison, it appears, that the three tables agree within less than $1''$, as far as 80° from the zenith: from 80° to 88° of zenith distance, the numbers in the French table exceed those in Bessel's, the excess being $2''$ at 84° , and $4''$ at 88° ; and with a single exception at 88° , (probably, judging from the character of the adjacent number, arising from an error of computation,) the refractions in the new table are nearer to Bessel's than those in the French table; but when the zenith distance is greater than 80° , the author considers the accuracy of the French table questionable, both on account of the hypothetical law of the densities, and because the quantity assumed for the horizontal refraction is uncertain.

After giving a few examples, illustrative of the use of the new table, the author inquires how far the refractions are likely to be affected by the term which it was found necessary to leave out, because the present state of our knowledge of the phenomena of the atmosphere made it impossible to determine the coefficient by which it is multiplied. For this purpose, the variable part of that term has been computed for every half degree, from 85° to 88° , and the results are exhibited in a table. From this it appears, that this coefficient, although considerably less than that of the preceding term, may still have some influence on the refractions at very low altitudes. The mean refraction in Bessel's table, and in the new table, can hardly be supposed to differ $2''$ from the true quantity, which would limit the coefficient in question to be less than one-tenth. It is a matter of some importance to obtain a near value of this coefficient; and it is probable that this can be accomplished in no other way, than by searching out such values of the two coefficients as will best represent many good observed refractions at altitudes less than 5° . If such values were found, our knowledge of the decrease of heat in ascending in the atmosphere would be improved, and the measurement of heights by the barometer would be made more perfect.

At the end of the paper is given a table of mean refractions for the temperature 50° Fahr. and barometric pressure 30 inches, at every degree from 0° to 70° zenith distance, and at every $10'$ from 70° to the horizon; and tables of the corrections requisite for variations of the thermometer and barometer are subjoined.

XXI. *Intelligence and Miscellaneous Articles.*

CAUSE OF THE CIRCULATION OF THE CHARA.

M. DONNÉ states that he thinks he has discovered the cause of circulation in plants, of which the *Chara* is a remarkable example. Contrary to the opinion of several authors who have attributed this circulation to physical agents, M. Donné supposed that its cause might be found in an organic disposition, and with this view entered into an examination, the result of which he thus states:—

“After having carefully taken off the outer coating of a tube of *Chara hispida*, and deprived it of the carbonate of lime, which interferes with its transparency, I submit it under the microscope to a methodical and graduated pressure, by the aid of M. Purkinje’s press. This pressure immediately detaches a great number of granules. Little strings, formed of 5, 6, or more granules, are then seen to put themselves in motion, turn round, and then stop if they are not carried away by the current of the fluid; other granules are completely detached from one another, and free from all adhesion; amongst these are seen some which have a rotary movement, more or less rapid, independent of the movement of general circulation; some turn round on themselves without changing place; others are carried along by the current, but still preserving their spontaneous rotary movement.

“These small bodies are therefore endowed with a peculiar power, which they obey when they are free, but which react on the liquid in which they are immersed when they are fixed.

“The rotary motion I speak of, is independent of that of the liquid in circulation; it is often very rapid indeed in comparison with the motion of circulation, and takes place when the circulation is slowest or has even ceased: it is not a rare case to see two granules near one another moving in a contrary direction; the following experiment will show this fact in a decisive manner.

“By pressing out the sap from a tube of *Chara* upon a piece of glass, and submitting this drop of liquid to inspection under the microscope, it is found to be composed, not only of the fluid and of white particles which were in circulation, but of a certain quantity of green granules, which the pressure has detached from the sides of the tube; the greater number of these granules are strung together, and no motion is to be discovered in them, nor in the free isolated granules spread over the surface of the glass. This is not the case with the large oily or albuminous drops which the interior fluid of the *Chara* forms upon diffusing itself: it is seldom that in some of these drops, the transparency of which is unfortunately troubled by a number of small grains, one or several green granules are not found endowed with the same spontaneous rotary motion which takes place in the interior of the tube itself; these granules being in their natural fluid have preserved all their properties, whilst the others may be considered as dead.

"It is impossible," continues M. Donné, "not to remark the striking analogy which these facts establish between the corpuscles ranged in regular and fixed series on the internal sides of all vegetable cells where the double circulation of a fluid has been observed, and the vibratile organs of animals, to which attention has been directed since the work of MM. Purkinje and Valentine: this analogy is the more complete, as the vibratile organs of the mucous membranes separate themselves, as I have shown, into particles, when the motion may be observed to continue often for more than twenty-four hours.

"I examined if there existed vibratory hairs on the surface of the granules endowed with the spontaneous movement which I have just described, but was not able to discover any, although I employed a power of 500 diameters with a good light. I thought I saw a brilliant circle round the granules, but cannot affirm any thing more on this point.

"I must add that all the agents which stop the circulation of the *Chara*, also destroy the rotary motion of the granules."—*L'Institut*, April 1838.

ACTION OF PLANTS ON THE AZOTE OF THE ATMOSPHERE.

M. Boussingault has entered into an investigation in order to ascertain whether plants absorb the azote of the atmosphere. In these researches he has employed analysis, and compared the composition of the seeds with the composition of the results of their growth, obtained at the expense only of water and air. Although these experiments were undertaken specially with a view to examine into the question as to azote, they also determine with precision the elements lost or gained by clover seed and wheat, during their germination and vegetation. These plants were vegetated in air continually renewed, and well washed to deprive it of all dust, watered with distilled water, and cultivated in a siliceous sand. The following are the results of this investigation.

1. That clover seed and wheat during germination neither gain nor lose azote.

2. That these seeds lose carbon, hydrogen, and oxygen, and that the quantity of each of these elements varies at different periods of their germination.

3. That during the culture of clover seed in a soil absolutely free from manure and only under the influence of water and air, this plant takes up carbon, hydrogen, and oxygen, and an appreciable quantity of azote.

4. That wheat cultivated under precisely the same circumstances also takes from the water and the air, hydrogen and oxygen; but after a culture of three months not the slightest gain or loss of azote could be detected by analysis.

The following is the result of M. Payen's examination on azote contained in plants.

That the radicals of all plants contain an azotic substance in sufficient abundance to give out upon distillation free or carbonated ammonia.

That every organ in a state of growth or in the course of development contains an abundance of an azotized substance; that as the organ becomes developed, the azotized substance decreases in quantity in comparison to the non-azotized substance, which latter becomes by degrees altogether predominant. From a number of examinations he concludes this fact to be general;—that the cambium contains this azotized substance in abundance, and that the sap is also charged with it.

M. Payen found in passing a quantity of water through a recently cut stick of elder that the wood was deprived of all the azotized matter, which the water separated; from this he is led to explain the action of all the substances employed to preserve wood which have the effects of acting upon the azotized substance, coagulating it, and rendering it insoluble in water.—*L'Institut*, February 1838. No. 224.

PROPORTIONS OF ANIMAL AND EARTHY MATTER IN HUMAN BONES.

Dr. G. O. Rees read a paper on the above subject, on the 8th of May, at a meeting of the Medico-Chirurgical Society.

The author, after alluding to the precautions necessary to be observed in the analysis of bone, proceeded to recount the results of his examinations. The solid parts of the femur, tibia, fibula, humerus, radius, and ulna were chosen for analysis; also the squamous portion of the temporal bone, the arch of a dorsal vertebra, the external crust of a rib and of the clavicle, the coracoid process of the scapula, a portion of the ilium near the crest of the bone, the metatarsal bone of the great toe, and a part of the middle portion of the sternum. These bones had been similarly prepared, and were quite free from fat, periosteum, and cartilage, and perfectly dry. The long bones of the extremities were found to contain from 63·02 to 60·01 per cent. of earthy matter, and the bones of the trunk from 58·79 to 54·51 per cent. The author then mentioned the general conclusions to which his experiments had led him. They were as follows:—

1st. The long bones of the extremities contain more earthy matter than those of the trunk.

2nd. The bones of the upper extremity contain somewhat more earthy matter than the corresponding bones of the lower extremity; thus the humerus more than the femur, and the radius and ulna more than the tibia and fibula.

3rd. The humerus contains more earthy matter than the radius and ulna, and the femur more than the tibia and fibula.

4th. The tibia and fibula contain, as nearly as possible, the same proportions of earthy matter, and the radius and ulna may be considered as alike in constitution.

5th. The vertebra, rib, and clavicle are nearly identical as re-

gards the proportion of earthy matter, the ilium containing somewhat more of earths, the scapula and sternum somewhat less, the sternum containing more earths than the scapula.

6th. The bones of the head contain more earthy matter than the bones of the trunk, as observed by Dr. J. Davy; but the humerus and other long bones approach very near in their proportion of earths.

7th. The metatarsal bones may probably be ranked with those of the trunk in proportional constitution.

The cancellated structure of bone is shown by the author to contain less earthy matter than the more solid parts of the same bone; the cancelli of a rib contained 4 per cent. less of earths than the solid external crust.

Several of the laws of relative proportion observed in the adult skeleton were shown to hold good in the fœtal bones*. Thus, the bones of the upper contain more earthy matter than those of the lower extremity.

The humerus contains more earthy matter than the radius and ulna, and the femur more than the tibia and fibula.

The ilium contains more, and the scapula less earthy matter than the clavicle or rib.

The great difference observable in the proportional constitution of the adult and fœtal bones, consists in the fact that the long bones and the bones of the head do not contain the excess of earths observed in the adult skeleton.

The author concludes, by showing that the bones of the trunk, in the fœtus, contain as large a proportion of earthy matter as those of the adult.

ON THE AMMONIACAL AND OTHER BASIC COMPOUNDS OF THE COPPER AND SILVER FAMILIES. BY PROFESSOR KANE.†

Having verified Berzelius' formula for the ammoniacal sulphate of copper $\text{cu so}_3 + 2 \text{nh}_3 + \text{ho.}$, Dr. Kane pointed out, that, from the circumstances of its formation, and others, the real formula must be $(\text{nh}_3\text{ho} + \text{so}_3) + \text{nh}_3.\text{cuo}$; and that by heat it loses $\text{nh}_3.\text{ho.}$ and leaves a compound $\text{nh}_3.\text{cu o} + \text{so}_3$; by still more heat there remains $2 \text{so}_3 + 2 \text{cu o} + \text{nh}_3$ or $\text{cuo}.\text{so}_3 + (\text{nh}_3.\text{cu o}) \text{so}_3$. and by water there is formed the ordinary basic sulphate $\text{cuo}.\text{so}_3 + 3 \text{cuo} + 4 \text{ho.}$

Dr. Kane describes likewise a new basic sulphate as $\text{so}_3 + 8 \text{cu o} + 12 \text{ho.}$ and he arranges these two salts as

$$1 = \text{cu o}.\text{so}_3 \text{ cu o} + 2 (\text{cu o} + 2 \text{ho}).$$

$$2 = \text{cu o}.\text{so}_3.\text{cu o} + 6 (\text{cu o} + 2 \text{ho}).$$

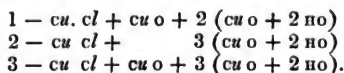
and seeks to establish an analogy with the ordinary salts of the same family, as

$$\text{zno}.\text{so}_3 \text{ ho} + 6. \text{ho} \text{ and } \text{cu o}.\text{so}_3.\text{cu o} + 6 \text{cu o}.$$

* The fœtal bones examined by the author were deprived of fat, periosteum, and epiphysis, and were perfectly dry.

† From the Proceedings of the Royal Irish Academy for May 28, 1838; with corrections by the author.

Dr. Kane found the ammoniacal chloride of copper to be $\text{cu cl} + 2 \text{nh}_3 + \text{ho}$. or correctly, $\text{nh}_3. \text{h cl} + \text{nh}_3. \text{cu o}$. By heat $\text{nh}_3 \text{ho}$ is lost, and there remains $\text{nh}_3. \text{h cu cl}$. By water there is generated a new basic chloride of copper, having the formula $\text{cu cl} + 4 \text{cu o} + 6 \text{ho}$. The common Brunswick green $\text{cu cl} + 3 \text{cu o} + 4 \text{ho}$. Dr. Kane has obtained with 6 ho in place of 4 ho. and these oxychlorides he considers as formed on the type of the ordinary chlorides, combined with water, or with metallic oxides in other groups.

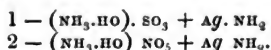


Dr. Kane has obtained another new oxychloride composed of $\text{cu cl} + 2 \text{cu o}$; taking one atom of water, it remains brown, but with more it forms a green powder,—the first replacing the third of cu o in the common oxychloride.

When No. 2 is heated, it loses all water, but if then put into contact with water, it regains 4 ho, and becomes perfect Brunswick green No. 1. $\text{cu cl. cu o} + 2 (\text{cu o} + 2 \text{ho})$.

The second equivalent of oxide is, in these chlorine bodies, much less forcibly held than in the sulphates, but that it is differently related to the acid than the remaining equivalents of oxide or of water is proved by a great variety of facts.

The ammoniacal nitrate of copper has the formula $\text{cuo no}_3 + 2 \text{nh}_3$. or $(\text{nh}_3. \text{ho.}) \text{no}_3 + \text{cu nh}_3$. hence this body contains, united with the copper, amidogen; when heated it explodes, the copper and amidogen burning in the nitrous oxyde yielded by the nitrate of ammonia. To obtain some analogical evidence regarding this body, Dr. Kane re-examined the ammonia-sulphate and nitrate of silver, and found George Mitscherlich's results good. Dr. Kane, however, writes the formulæ



This last salt, when heated, gives a beautiful decomposition; the nitrate of ammonia fuses readily, and at a temperature below that at which it decomposes, the amide of silver is resolved into ammonia, nitrogen, and metallic silver, which latter being deposited on the sides of the glass, from the liquid nitrate of ammonia, gives a mirror surface equal to that obtained by aldehyd.

On analyzing the ammoniacal compounds of nickel, Dr. Kane found the results of Erdman completely verified; but from the inferior affinity with which the ammonia was retained, these compounds did not yield as positive results as to their influence on theory, as those of the copper class.

A new substance, discovered in the course of these researches, may be termed a fulminating copper. It is a blue powder, decomposed by heat into metallic copper, water, ammonia, and nitrogen. Its formula is $3 \text{cuo} + 2 \text{nh}_3. + 6 \text{ho}$.

The examination of the zinc compounds has led to the discovery

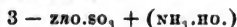
of a considerable number of new bodies. The ammoniacal sulphate of zinc crystallized is



exposed to the air it effloresces, losing HO, and becomes



which, if heated, gives at 212°F .

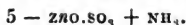


but at dull redness loses still $\text{NH}_3.\text{HO}$ and leaves $\text{zno}.\text{so}_3$.

If No. 1 be exposed longer to a moderate heat it loses 2 HO. and there remains,



If this be heated to 300° , it loses $(\text{NH}_3.\text{HO})$ and there is



which further gives by heat



from which the ammonia cannot be expelled without decomposition.

Selecting from among these No. 2, for reduction to its rational formula, it becomes



Now the oxide of zinc from the sulphate being redissolved by potash, there must be formed the similar compound



This cannot be obtained crystallized, for if the liquor be evaporated there is deposited $\text{KO}.\text{so}_3$, and $\text{zno}.\text{KO}$ remains dissolved; from this, by exposure to the air, there are gradually deposited small crystals, which Dr. Kane considers as being



but by heat there is carbonic acid given off, and a powder insoluble in water is produced, the composition of which, from Dr. Kane's examination, appears to be



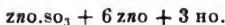
It will be recollected, that the bicarbonate of potash is



By treating the ammonia sulphate No. 3 or 5 by water, there is obtained a basic sulphate, having the formula



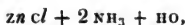
which, dried and exposed to the air, slakes, and gives



This new salt has some remarkable relations to those already known.

There are two ammonia chlorides of zinc.

No. 1, in pearly scales of a talcy lustre, consists of



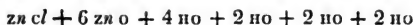
and, when heated, gives off $\text{NH}_3.\text{HO}$. leaving $\text{NH}_3.\text{zn}.\text{cl}$. a white powder.

No. 2 is in fine quadrangular prisms, brilliant lustre, consisting of $2 \text{ zn cl} + 2 \text{ nh}_3 + \text{ho}$. or as Dr. Kane considers, $\text{zn.cl} + (\text{nh}_3.\text{hcl}) + \text{nh}_3.\text{zno}$. which losing $\text{nh}_3.\text{ho}$ leaves $\text{zn cl} + \text{nh}_3.\text{zn cl}$. a white mass, fusible, congealing into a mass like gum, and volatilizable. This gummy mass is likewise obtained by heating $\text{nh}_3.\text{zn cl}$.

There is generated by the action of water on these basic ammoniacal compounds, an oxychloride of zinc of a very remarkable character; it is—



dried, it is reduced at 212° to $9 \text{ ho} +$ and by 300° to 6 ho . By 500° all water is driven off, and there remains $\text{zn cl} + 6 \text{ zn o}$ which exposed to the air absorbed 3 ho . Hence the general expression is



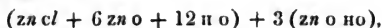
and comparing some similar chlorides, there is,

- 1 $\text{ca.cl} + 6 \text{ ho}$ crystallized chloride of calcium.
- 2 $\text{zn cl} + 6 \text{ zn o}$ — basic chloride of zinc dry.
- 3 $\text{h cl} + 6 \text{ ho}$ — strong muriatic acid.
- 4 $\text{zn cl} + 6 \text{ zn o} + 10 \text{ ho}$ — hydrated oxychloride of zinc.
- 5 $\text{h cl} + 6 \text{ ho} + 10 \text{ ho}$ — muriatic acid with a constant boiling point.

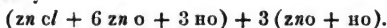
Another oxychloride, having the composition



which dried and exposed to air, absorbs 6 ho . Hence it may best be considered as



giving ultimately



METEOROLOGICAL OBSERVATIONS FOR JUNE 1838.

Chiswick.—June 1. Very fine: rain. 2, 3. Cloudy and fine. 4. Fine: rain. 5. Hazy: very fine. 6. Slight haze: fine. 7. Fine. 8. Cold and dry. 9. Fine. 10. Cloudy and fine. 11. Hazy: rain. 12. Rain, with thunder. 13. Fine: heavy rain at night. 14, 15. Overcast: rain. 16. Drizzly. 17. Cloudy. 18. Overcast: heavy thunder showers. 19. Cloudy and windy. 20. Cloudy: boisterous with rain at night. 21. Cloudy and windy. 22. Showery. 23, 24. Very fine. 25. Fine; heavy rain. 26. Hazy: rain. 27. Overcast and fine. 28. Slight showers: very fine. 29. Fine: heavy rain. 30. Fine: rain.

Boston.—June 1. Rain. 2. Fine. 3. Fine: rain A.M. 4. Fine: rain, thunder, and lightning A.M. 5. Fine: heavy rain and hail, with thunder and lightning P.M. 6. Cloudy. 7. Cloudy: rain P.M. 8, 9. Fine. 10. Cloudy. 11. Rain. 12. Fine. 13. Rain early A.M. 14. Cloudy. 15. Fine: rain P.M. 16. Fine. 17. Fine: rain early A.M. 18. Fine. 19. Cloudy: heavy rain early A.M. 20. Rain. 21, 22. Cloudy. 23—25. Fine. 26. Cloudy: rain A.M. and P.M. 27. Cloudy: rain P.M. 28. Fine. 29. Cloudy: rain early A.M. 30. Fine: rain P.M.

Days of Month, 1888.	Barometer.				Thermometer.				Wind.				Rain.			Dew-point.			
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8 1/4 a.m.	Dumfries-shire. 9 a.m.		London: Roy. Soc. 9 a.m.	Self-register. 9 a.m.	Fah. 9 a.m.	London: Roy. Soc. 9 a.m.		Chiswick.	Max.	Min.	Dumfries-shire. 9 a.m.		Chiswick.	Boston.	Dumfries-shire.
		Max.	Min.		Max.	Min.				Max.	Min.								
1.	29.974	30.032	29.886	29.55	60.8	70.3	52.7	70.3	52.7	NW.	S.	50				.372	.27	.22	56
2.	29.836	29.908	29.770	29.36	59.3	70.3	52.5	70	47	S.	SW.	56				.644	.08	.04	55
3.	29.800	29.750	29.652	29.26	59.5	66.0	51.3	69	49	S.	SW.	61				.341	.04	...	55
4.	29.732	29.833	29.743	29.21	61.5	66.6	53.6	72	40	S.	SW.	61				.05505	54
5.	29.924	29.964	29.903	29.37	58.6	70.5	50.4	74	45	SSW.	W.	61				.02219	54
6.	30.052	30.108	30.085	29.55	52.7	69.3	51.0	57	43	NW.	NE.	54			21	52
7.	30.212	30.264	30.157	29.72	54.9	58.7	50.6	64	40	NW.	N.	54				51
8.	30.304	30.322	30.247	29.82	51.7	64.3	44.2	66	35	NW.	N.	57			08	42
9.	30.240	30.290	30.021	29.24	59.0	70.3	47.0	68	43	SW.	S.	57				48
10.	29.798	29.850	29.580	29.36	59.3	64.2	50.5	66	51	SW.	S.	58				50
11.	29.518	29.590	29.493	29.12	55.2	63.0	52.3	67	49	NE.	E.	60				.227	.47	.04	53
12.	29.564	29.677	29.593	29.12	55.7	60.8	52.4	72	42	S.	W.	60				.533	.02	...	54
13.	29.682	29.750	29.677	29.22	56.8	62.8	50.8	69	47	NW.	E.	53				.366	.56	.19	53
14.	29.744	29.800	29.678	29.20	59.8	63.0	51.2	67	52	S.	W.	66				.100	.02	.02	54
15.	29.784	29.844	29.678	29.25	60.7	64.5	54.0	73	49	S.	W.	62				.025	.02	...	55
16.	29.800	29.850	29.810	29.23	61.8	65.6	54.8	76	58	S.	W.	66				.013	.17	.11	54
17.	29.888	29.948	29.747	29.26	63.5	70.4	60.3	75	58	S.	W.	66				.172	.02	.06	60
18.	29.682	29.678	29.562	29.06	68.3	71.5	61.0	76	54	ENE.	S.	69				.022	.25	...	62
19.	29.826	29.905	29.849	29.08	63.8	71.7	54.7	72	53	S.	SW.	61				.177	.37	.39	58
20.	29.736	29.792	29.583	29.20	60.8	67.2	59.0	72	51	SW.	SW.	59				.283	.05	.27	60
21.	29.590	29.701	29.640	28.96	62.4	72.3	53.6	71	46	W.	SW.	63				.033	.01	...	58
22.	29.860	29.984	29.857	29.21	62.4	72.6	51.7	77	45	SW. var.	SW.	63			12	...	57
23.	30.192	30.212	30.099	29.55	66.7	79.7	54.8	83	55	NW.	S.	67				.227	56
24.	30.052	30.115	30.008	29.44	68.3	77.3	61.6	79	56	NW.	S.	66				59
25.	30.090	30.142	30.007	29.46	68.3	77.3	61.6	79	56	E.	E.	67				61
26.	30.000	30.060	29.962	29.40	57.4	72.8	56.0	68	53	NNW.	N.	62				.572	.01	...	60
27.	30.044	30.090	29.993	29.42	65.0	74.6	57.0	65	54	N.	NE.	60				.019	.01	.31	59
28.	30.050	30.100	29.948	29.46	62.9	66.3	57.3	73	49	SW.	S.	64			04	...	58
29.	29.998	30.050	29.959	29.46	62.8	72.8	54.3	74	44	W.	SW.	58			20	.37	57
30.	29.974	30.028	29.844	29.40	62.8	72.7	52.0	74	52	S.	W.	62				.169	.07	.27	56
Mean.	29.898	29.921	29.839	29.33	60.5	68.6	53.6	70.83	48.96	59.8						Sum.	3.65	2.86	Mean.
																			55.4

The Meteorological Observations from Applegarth Manse, Dumfries-shire, for June, have not yet been received.

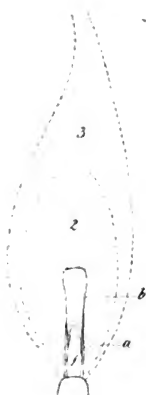


Fig. 1.

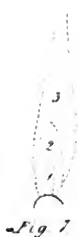


Fig. 7.



Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



Fig. 6.

DEPOLARISATION OF HEAT.

Fig. 1. Hyaline Lamin.

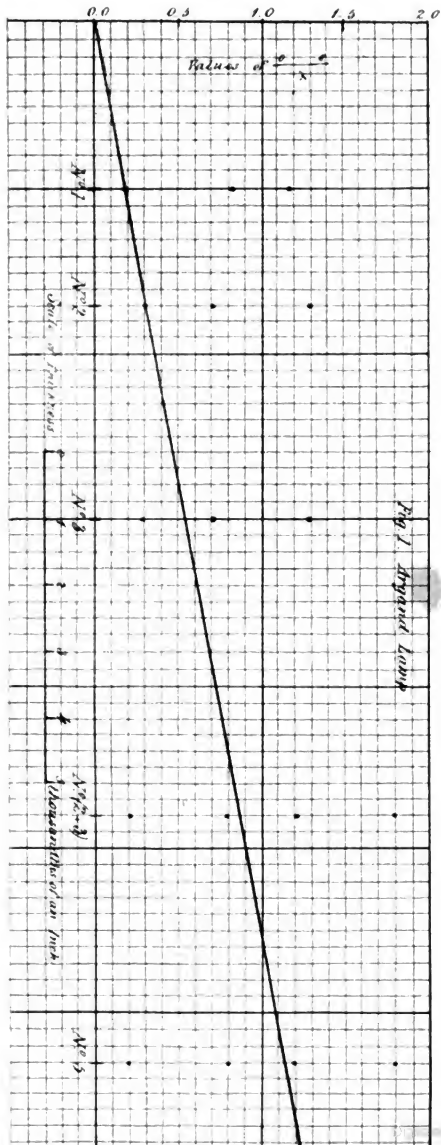
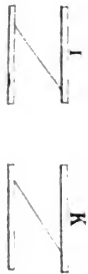


Fig. 2.



Screen

Source

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Fig. 3.



Mr. Richardson's Method of Analyzing Coal.

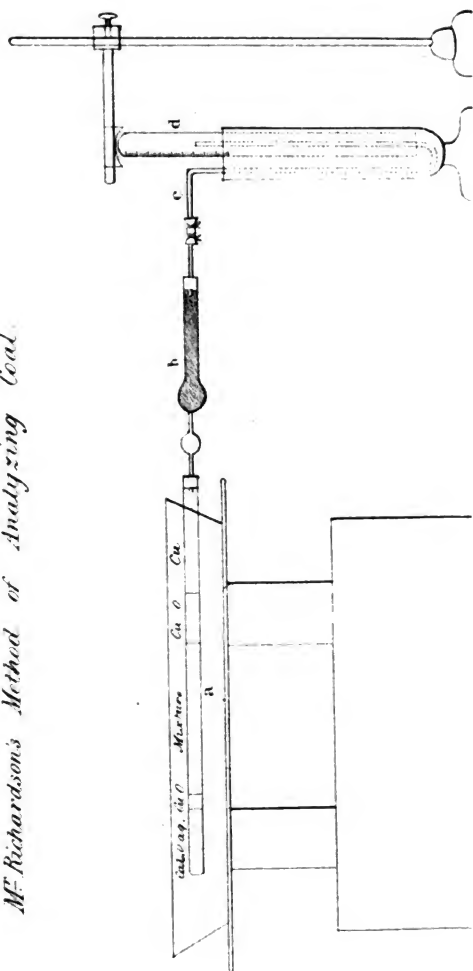


Fig 1.

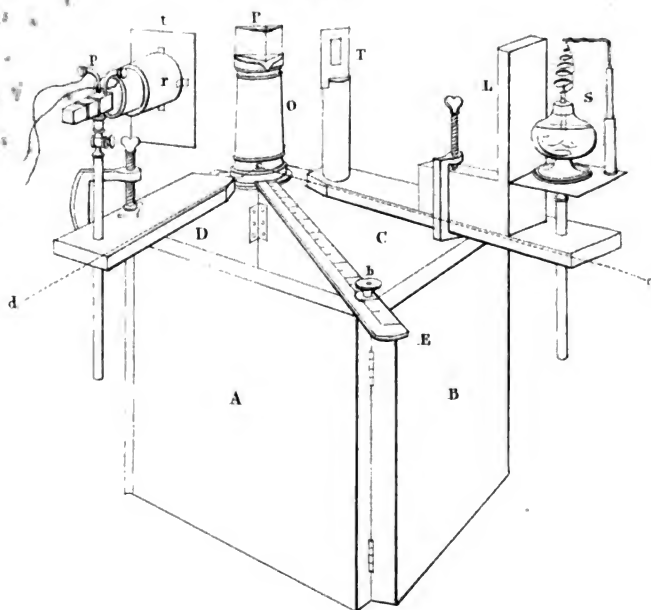
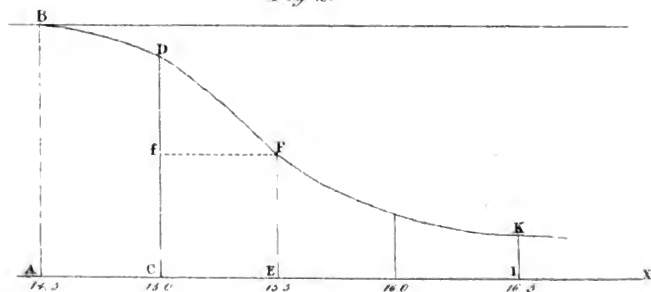


Fig 2.



THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

SEPTEMBER 1838.

XXII. *Discussion of M. Fechner's Views of the Theory of Galvanism, with reference, particularly, to a circuit including Two Electrolytes, and to the relations of Inactive Iron.* By Prof. SCHENBEIN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the letter I had the honour of addressing to you some months ago, I mentioned *en passant* that the chemical theory of galvanism was about to be severely attacked by some German philosophers. I now perceive by the 12th number of Poggendorff's *Annalen* for 1837, that M. Fechner has laid before the scientific public his objections to this theory, in a paper entitled "Justification of the Voltaic Theory."* Having many reasons to suspect that the controversy respecting the source of voltaic electricity will before long be resumed with more ardour than ever, and being besides almost sure that the results of my late researches regarding the voltaic action of metallic peroxides will be made use of as a piece of evidence in favour of Volta's views, I think it not quite unseasonable to offer a remark or two upon some of the objections brought forward by M. Fechner. As to the assertions made by that philosopher with regard to some fundamental experiments of De la Rive, I do not feel myself called upon to refute them, as no doubt the distinguished electrician of Geneva is himself best able to maintain the points attacked. The remarks I am going to make will principally bear upon the conclusions which have been drawn by M. Fechner from the results of the following experiment.

* We commence in the present number, p. 205, a translation of M. Fechner's paper here mentioned.

Ten pairs of zinc and copper, in every respect as equal to one another as possible, were arranged into a "*couronne des tasses*," so that half of the said pairs produced a current opposite in its direction to that which was originated by the other half. The exciting fluid used was common water. Such an arrangement being connected with the galvanometer, can, according to either of the two principal theories of galvanism, have no effect upon the needle, provided everything in the two systems of cells be equal. Fechner, after having obtained current equilibrium in the manner described, put muriatic acid into one of the above-mentioned systems, and found that in these circumstances the previous equilibrium was in the first instance maintained, but that by degrees the current of the water-cells got the ascendancy over that of the acid system. Fechner thinks these results to be quite irreconcilable to Faraday's theoretical views on galvanism, and is inclined to consider his experiment as an "*experimentum crucis*" against the very first principle of the chemical theory of the voltaic phænomena. Before judging of the validity of Fechner's conclusions, I must not omit to give an account of some of the results which I lately obtained from a great many experiments made upon the subject under discussion.

1. Ten equal pairs consisting of zinc and copper were arranged in the manner before mentioned, and the vessels holding the former charged with common water. On closing the circuit by the means of a most delicate galvanometer (provided with 2000 coils), the needle deviated a little, but after a very short time it returned to zero. Current equilibrium being thus obtained, it was disturbed again by breaking and re-establishing the circuit through the means of any pair of either system. The deviation of the needle amounted to about 20° , and was always such as to indicate the superiority of that current which was produced by the set of pairs left untouched. The equilibrium, however, was also in this case very soon re-established after the closing of the circuit.

2. Equilibrium taking place in the arrangement just described, I added $\frac{1}{2000}$ of common sulphuric acid to the water of one set of the cells. On closing the circuit by means of the galvanometer, the needle of the instrument was made to deviate about 180° in such a direction as to show the ascendancy of the current excited by the acidulated water. Leaving the circuit closed for a few minutes, the needle however took up its usual position. Opening and closing the arrangement again by the means of any pair of plates belonging to the cells charged with the acid fluid, or doing the same by the means of the platina ends of the galvanometer-wire, had not the least

effect upon the needle; but on removing any pair of the water-cells from the circuit and replacing them there, the equilibrium was disturbed in favour of the acid system. But what seems to me to be still more remarkable, is the fact, that the same effect, only on a smaller scale, was obtained by making any pair belonging to the water system alternately rise and sink a little without removing it entirely from the circuit. Another fact worthy of being stated is, that immediately after the equilibrium had been disturbed, opening and closing the circuit by the galvanometer, or by any pair of the acid system, causes a similar effect, that is to say, a perceptible increase of the acid current. The arrangement left to itself closed regains, however, its previous condition within a very short space of time, i. e. assumes such a state of current equilibrium as can only be changed again by breaking and re-establishing the circuit by a pair belonging to the water system. It is also worthy of remark, that the magnitude of the differences of currents obtained by the means mentioned is variable. I have repeated the same experiment over and over again, and at each time I got a fresh result as to the number of degrees of the needle's deviation; sometimes even, no deviation at all took place. On making use of an aqueous acid fluid containing 1 part of sulphuric acid in one system of the cells, whilst there was common water in the other, I obtained a deviation in favour of the acid current which amounted to about 90° : in a very short time the needle, however, returned to zero. A fluid containing $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, $3\frac{1}{2}$, 4, $4\frac{1}{2}$, 5 parts of acid did not affect the needle; from $5\frac{1}{2}$ —8 parts produced a temporary deviation of about 40° in favour of the water current; from 9—11 parts had no effect; 15 parts caused a considerable deviation in favour of the acid current, but lasted only for a few moments; 20 parts caused no effect upon the needle.

3. A liquid containing for 100 parts of water 1—10 parts of common muriatic acid, and used in one system of the cells, did not cause any deviation; 15 parts caused a deviation of the needle of 40° in favour of the water current: after a short time the former returned to zero, but was again made to deviate in the same manner as it was at first. This change of equilibrium into difference of currents, and *vice versa*, took place several times. From 20—25 parts of acid caused a deviation of about 45° in favour of the water current, which lasted rather a long time; 30 parts of acid made the needle turn round its central point several times in such a direction as to indicate the prevalence of the water current. The same experiment made another time showed no difference of currents, the needle remaining at zero.

4. Water containing for 100 parts 5—20 parts of common nitric acid did not disturb the needle at all; 25 parts produced a rather long continuing deviation of about 50° in favour of the water current; 30 parts had the same effect, though the difference of currents was a little smaller. If in the latter experiment the two systems of pairs were made to change their cells, so that the set of pairs having previously been in water were placed in the acid-cells, and *vice versa*, a current equilibrium was obtained. In most of the fore-mentioned cases, where the needle remained at rest, the pairs of the water-cells could have their place supplied by platina wires, without thereby disturbing the equilibrium; and even if a deviation of the needle took place (immediately after having effected such a change), it was always very insignificant, and the equilibrium of currents was speedily re-established.

From the results of the experiments described, it appears that only in a few instances the chemical difference of the exciting fluids contained in the two systems of cells determines a difference of currents produced by the two sets of pairs, and that the general rule is the production of current equilibrium.

Now, if I have correctly understood Fechner's statements, they imply the assertion, that on using water in one system and an acid liquid in the other system of cells as exciting fluids, and everything else being equal in the arrangement, the equilibrium which takes place in the first instance is always by degrees disturbed in favour of the water current. If such were the case, the fact, as it seems to me, would be entirely contradictory to the theory of Volta; for according to the views of this philosopher the current produced by the one set of pairs must be equal to that which is excited by the other set, and the addition of acid to one system of cells has no other effect than to increase the conducting power of the whole arrangement. I have already remarked, that in the circumstances mentioned current equilibrium is the rule, the contrary an exception to it, and that even if a difference of currents occurs, it is generally so insignificant as to be made only perceptible by the means of a most delicate galvanometer. We may therefore consider the facts to be in perfect accordance with the theory of Volta. But does not the current equilibrium in question disagree with that theory which makes the production of current electricity dependent upon chemical action, and the quantity of the former upon the extent of the latter? I think it does not, and hope to be able to prove the correctness of my assertion by what I am about to say.

On the first view of the case, it seems, indeed, as if the fact

under discussion were not at all favourable to the chemical theory, for undoubtedly the chemical action which takes place in the acid cells is, as to extent [intensity?], infinitely superior to that which is going on in the water cells. There should, therefore, be a difference of currents proportionate to the difference of the extent of the chemical actions taking place in the two systems. Certainly, if we do not take into account the different degree of resistance which is offered to the circulation of the currents by the two sets of cells, the equilibrium in question must appear entirely at variance with the principles of the chemical theory, and speak in favour of Volta's hypothesis; but by duly appreciating the circumstance alluded to, the theoretical difficulty and the anomalous character of the fact can easily be removed.

According to the theory of the voltaic pile, such as it was established some time ago by M. De la Rive, the electricities which are set free by chemical action at the two ends of a closed compound circle unite themselves by two ways; one of which is the pile itself, the other the conductor placed between the poles. The quantities of the electricities recombining within each of the two conducting mediums depend, according to the same theory, upon the peculiar degree of the conducting power of each medium. Now let us at first consider only the acid cells as originating a current, and those charged with water merely as a medium put between the poles. It is manifest, that under such circumstances, by far the larger portion of electricities being developed by the pile, must reunite within the latter, and only a small quantity will consequently pass through the galvanometer and the water cells. If, as above stated, the latter are connected with one another by the means of platina, no current whatever circulates through the galvanometer, however violent chemical action may be within the water cells; but if pieces of copper or of any other readily oxidable metal are made use of instead of platina, there will pass a weak current from the acid system into that of water. This result is easily accounted for by the well-known fact, that those metals offer much less resistance to a current than platina does. From such being the case, it follows, that we should always obtain a current of the description mentioned, if the pairs of zinc and copper, by means of which the water cells of our arrangement are connected with one another, only acted the part of conductors. We know, however, that they also give rise to a weak current, which current, on account of the peculiarity of the arrangement, must be, as to direction, opposite to that excited by the acid cells. From the fact that in most cases above stated equilibrium takes

place, we must infer that both currents in question are generally equal to one another. The equality of the currents resulting from our two systems of cells is, no doubt, in some way or other connected with the fact, that two piles containing the same number of equal pairs, but being charged with different exciting fluids, exhibit in general no difference of tension at their insulated poles. As De la Rive has already discussed that point in his memoir entitled "*Recherches sur la Cause de l'Électricité voltaïque*," I do not think it necessary to enlarge upon it any further.

It appears to me that the preceding remarks are sufficient to demonstrate that the current equilibrium which results from the peculiar arrangement which has been mentioned of two compound circles (only differing from each other with regard to their respective exciting fluids) is by no means contradictory to the principles of the chemical theory, no more than, for instance, the fact is, that a pile consisting of ten voltaic pairs, half of them put into water cells, the other half into acid ones, and arranged in the usual way, produces a current much weaker than that which is obtained from five pairs alone placed within the acid fluid. For the same reasons which make Fechner consider the equilibrium in the first case as an evidence against the correctness of the chemical theory, he must draw similar conclusions from the results of the second case; for he may ask, why should the voltaic effect of ten pairs be smaller than that produced by only five pairs? as there can be no doubt that the extent of the chemical action of the whole arrangement is greater than that of only a part of it. After what has already been said about the subject, it would be quite superfluous to answer such a question.

As to the differences of currents mentioned in the beginning of this paper, those differences being sometimes in favour of the water system, sometimes of that of the acid system, I am inclined to think them connected with certain changes which the pairs functioning in the pile undergo with regard to their conducting power, though I am not able as yet to assign the ultimate cause of the modifications in question. There can, however, hardly be entertained a doubt about the occurrence of such changes; and to prove the correctness of the assertion, I have only to mention iron, which being in its peculiar condition, proves to be a very bad current-conductor, compared to what it is in this respect when in its ordinary state.

Dr. Faraday, in his comments upon my first letter addressed to him, (*L. & E. Phil. Mag.*, vol. ix. p. 60.) says that the voltaic relation of inactive iron to platina afforded a decisive proof, that contact of itself, independent of chemical action, is inca-

pable of producing current-electricity. I myself have drawn a similar conclusion from a series of facts, which I made known through the *Phil. Mag.* some time ago.

Fechner now asserts that the phenomena alluded to do not prove the least thing in favour of the chemical theory. Although the results of my late researches have, indeed, shown (see *L. & E. Phil. Mag.*, No. 74, 1838,) that inactive iron, being voltaically associated with platina and put into nitric acid, produces a weak current, which, as it seems, is quite independent of any chemical action, I nevertheless maintain that Faraday and myself were fully entitled to draw the inference mentioned. Hereafter I shall give my reasons for doing so. In support of his sweeping assertion, Fechner says that he and Wetzlar had satisfactorily proved, that the modification which iron undergoes in nitric acid renders that metal more negative than it is in its natural state. The fact that a highly negative metal neither precipitated copper from a solution of blue vitriol, nor was affected by nitric acid, nor produced a perceptible current when voltaically combined with platina, could not therefore be considered as irreconcilable with Volta's theory, &c. With all deference to M. Fechner's great abilities and merits as a philosopher, and particularly as an electrician, I cannot make up my mind, as already stated, to submit myself to his judgement. As to Mr. Faraday, I do not know whether he is prepared to acknowledge the fallacy of his reasonings, but I strongly doubt [whether] he is. The reasons which determine me to insist upon my former opinions are as follows.

The assertion of M. Fechner, according to which iron, by assuming its peculiar condition, becomes a highly negative body, has, if translated into the language of the chemical theory, no other meaning than this;—that iron becomes a metal less oxidable than it is in its natural state. Fechner's view of the case implies to a certain degree a hypothetical explanation of the non-oxidability of iron, whilst I confine myself to stating facts, and the order in which certain phenomena succeed to one another. But let us for a moment grant the existence of electrical relations of bodies to one another, such as supposed by the voltaists; and if we further admit that by some means or other, for instance, by a peculiar action of nitric acid, iron can be changed from an electro-positive metal into a negative one, it is to be asked why such a change is effected by making common iron the anode of a current. According to my experiments, iron, whilst acting the part of the positive electrode of a pile, does not throw down the smallest particle of copper out of a solution of blue vitriol, and allows oxygen to be disengaged just as platina does. Agreeably

to the same experiments, the circulation of the current has no sooner been interrupted than the iron is acted upon by the copper solution in the usual manner. On using any other aqueous solution of oxi-acids or oxi-salts, which in ordinary circumstances act chemically upon iron, we obtain the same results. Now in accordance with his views, Fechner is forced to admit, in order to account for the inactivity of the metal, that iron by acting as the positive pole of a pile is rendered an electro-negative body. I must confess it appears utterly impossible for me to conceive how the admission of such a state of things can be reconciled to the principles of Volta's theory, and the electro-chemical systems of our days. According to my humble opinion, the very reverse of what is really the case should take place, that is to say, iron performing the function of the positive pole of a pile, ought to become a metal more positive, or what comes to the same, more oxidable than it is in its usual condition. Is it possible that the same particles of a body are at the same time in two opposite electrical states, or can any substance at the same time attract and repel oxygen? It is quite obvious that Fechner's assertion implies the admission of such a state of things; for the disengagement of oxygen from the iron must be considered, according to his views, as the effect produced by two different causes: first by that metal being the positive electrode, and then by its (the iron) being a highly electro-negative body. For my part, I cannot adopt such an extraordinary opinion, and must consequently consider as erroneous Fechner's assertion, according to which the peculiar condition of iron depends upon the metal having changed what is called its natural electro-chemical relations.

I certainly do not pretend to know in what manner a current changes the natural qualities of iron; but my ignorance on this point does not force upon me a hypothesis so ill suited to the principles of the chemical theory as the one spoken of appears to be, with regard to the voltaic-electro-chemical system which is maintained by Fechner.

Before passing to another subject, I have still a remark or two to make respecting the matter in question. If a piece of copper (in the shape of a wire,) and one of inactive iron are connected on the one side with a galvanometer, and on the other with a solution of blue vitriol, the needle will be made to deviate in such a direction as to indicate a current passing from the copper through the fluid into the iron. Such a state of things lasts, however, only for a few seconds, the current quickly changing its direction, and the iron becoming positive with regard to copper. This change occurs at the very same

moment when the former metal ceases to be in its peculiar state. Similar phenomena take place, if in the experiment, instead of a solution of copper, nitric acid (not too strong) is made use of; but no such results are obtained, when, in place of copper, a metal is substituted which is not chemically acted upon by the fluids mentioned. It is certainly true, that inactive iron being put into a copper solution becomes of itself active; but if the peculiar condition of the metal has been called forth by repeated immersions in nitric acid of 1.35, by which means the highest degree of stability of chemical inactivity is excited, it can stand rather a considerable length of time in such a solution before it becomes active; whilst, as already stated, the peculiar condition of iron is almost instantaneously destroyed, if the metal be voltaically associated with copper or any other of the more readily oxidable metallic substances. It is hardly necessary to mention here, that copper is chemically acted upon by solutions of the deut-salts of that metal. Now if, according to M. Fechner's opinion, inactive iron be a negative metal, how does it happen that in the circumstances mentioned, the iron changes so suddenly its voltaic character and turns positive again; and how comes it that this change of state takes place only in case the metal, which is voltaically combined with the iron, acts chemically upon the fluid into which both metallic substances are plunging? I account for the change in question in the following manner. My experiments have proved, that inactive iron by being made the cathode of a current of a certain strength loses its peculiar state, i. e. turns active. Now if such inactive iron be voltaically associated with copper for instance, and both metals put into a solution of blue vitriol, the copper will be oxidized, and by this means a current excited to which the iron of the arrangement bears the relation of the cathode. This current must, according to what I said before, destroy the peculiar condition of the latter metal and throw it into chemical action. This action being superior to that which takes place at the copper, the current produced by the former must also surpass in strength the current which is excited by the latter action; and hence it follows that iron must become positive with regard to copper.

As Fechner does not allow chemical action to be a source of current electricity, he of course cannot take the least notice of what is going on in the voltaic arrangement described in a chemical respect, and he must find out some other cause in order to account for the change of the voltaic relations of both metals to one another. What this cause may be, I must confess I have not the least idea of.

There is another fact that bears upon our question and

about which I must offer some remarks. If two pieces of iron wire, one of which is to be inactive, the other in its natural condition, are connected with a galvanometer, and the free end of the former first put into common nitric acid, and afterwards the free end of the ordinary wire, it is well known from my former experiments that the latter piece of wire becomes inactive, and that at the same time a current is produced which passes from that wire through the acid into the inactive one. The latter wire, therefore, is to the common one as platina is to zinc. Such a state of things, however, lasts only for a few moments, the voltaic difference of the two iron pieces disappearing very quickly. If in the experiment described the ordinary iron wire be first plunged into the acid, the inactive one is thrown into chemical action, and a current excited, to which the latter wire acts the part of the cathode; but no sooner has the passive iron been rendered active, than the current ceases, or rather current-equilibrium is established. Now if chemical action has nothing to do with the production of current electricity, why does the order in which our wires are plunged into the acid determine the results spoken of? According to Fechner's views, inactive iron ought to remain in its peculiar condition, whether it be put before or after the ordinary wire into the fluid; and the more should this be the case, that the Saxon philosopher does not seem to admit of the existence of any causal connexion between a current and the inactivity of iron, making the latter depend upon a peculiar action of nitric acid or nitrate of silver on that metal. I scarcely need to mention, that the same principle made use of to account for the phænomena to which iron associated with copper gives rise in a solution of blue vitriol, is perfectly applicable to the facts just now stated; and I should think that, according to the present state of science, no other account of them can be given. In the experiments described, chemical actions and voltaic effects appear, indeed, so closely connected with one another, that an unbiassed mind can hardly help considering the first as the cause of the latter.

I have perhaps enlarged too much upon my subject, and am really afraid of being chargeable with prolixity; but as the discussion into which I have entered regards the very first principle of the chemical theory of galvanism, I thought I could render some service to science by minutely appreciating the value of the objections which have been brought forward with the view of invalidating or rather overthrowing the chemical theory.

Up to this present moment I have considered the definite action of an electric current, and the fact, that the quantity of the latter produced by an hydro-electric arrangement is deter-

mined by the quantity of metal oxidized, as the most conclusive proof of the dependence of current electricity upon chemical action; but to my great surprise, I am now given to understand that those important facts do not prove anything at all in favour of the chemical theory, though I have not yet been so happy as to meet with even the slightest attempt to explain them according to the principles of Volta's hypothesis. Certainly, if Faraday's beautiful discovery is rejected as an evidence, we must despair of finding out another of more weight; and I am afraid any further discussion upon the subject with our antagonists will prove perfectly useless and a mere waste of time on our part.

I am, gentlemen, yours, &c.

Bâle, 14th June, 1838.

C. F. SCHËNBEIN.

XXIII. *On some of the Phenomena and Laws of Action of Voltaic Electricity, and on the Construction of Voltaic Batteries, &c.* By CHRISTOPHER BINKS. *A Second Communication, addressed to J. F. Daniell, Esq., F.R.S., &c., Professor of Chemistry in King's College, London. Part the First*.*

[Continued from p. 145.]

Section VI.

TO determine the comparative amount of voltaic action induced in any single arrangement under different distances of the two elementary plates from one another, continued.

115. The preceding part of the inquiry relates to the influence exercised by distance upon a voltaic arrangement, in which the elementary plates are of an *equal* size relatively; and it has been shown (84 to 92) that of whatever magnitude the couple itself may be, so long as its two plates are equal in size one to the other, the influence of distance is the same as has been stated in the preceding section.

116. It is now required to find the comparative effects of distance in any arrangement, when the elementary plates are of an *unequal* size relatively, but whilst all other conditions of the experiment remain the same as before.

117. The elementary plates may differ from one another in size to any extent: the zinc plate may be a small one, and the copper plate be made the larger, and that to an unlimited extent; or, on the other hand, the copper plate may be made the smaller of the two, and the zinc one be increased in size to an unlimited extent.

* This portion of Mr. Binks's Second Communication was by mistake termed Part the Second in our last.

118. But in the instances that immediately follow, the experiments will be restricted to that condition of the arrangement in which the zinc plate is the smaller of the two.

119. The plates used in the preceding experiments were each one inch square (93); but one side of the zinc being covered with wax, was not brought into action, whilst both sides of the copper plate were exposed to the action of the acid throughout: the total area of the zinc surface was therefore equal to one square inch, and that of the copper to two square inches.

120. The experiments to determine the further effects of distance are now to be varied by the use of larger copper plates, whilst the magnitude of the zinc plate remains precisely the same as before.

121. The set of copper plates prepared for these and the subsequent experiments amounts in number to 13, beginning from the size of 2 square inches of total surface, as in the one above, and progressively increasing by the following rate:—

122. Total superficial area in square inches of each copper plate.

2. 4. 8. 12. 18. 24. 32. 40. 50. 60. 72. 98. 128.

123. Each plate was attached to a wire 5 feet in length, in the same manner as before. The various precautions used in preparing this set of plates, and in adapting them to their present use, have already been fully explained (62).

124. Now the immediate object of the inquiry is to ascertain what influence is exercised by distance upon any arrangement which may be composed of a zinc plate having a total superficial area of 1 square inch, and of a copper plate whose area is any of those just stated (122). The relative distances contemplated range between $\frac{1}{4}$ of an inch and 48 inches; and to complete the investigation, the acid mixture should be varied in strength in the four different degrees used in the former experiments.

125. A little consideration will show how innumerable and varied the experiments would need to be, in order to complete an investigation which should undertake to determine the effects of distance under all the varied changes of the conditions of such arrangements of which they are capable.

126. But it will perhaps be unnecessary to pursue the investigation to so great a length. Such results as it may be desirable to seek for will perhaps be obtained by a very few observations comparatively; and as most likely to contribute to this abridgement, the order of the experiments as they now follow was determined upon.

127. The amount of action yielded by any voltaic arrange-

ment will be increased by increasing the size of either plate of the couple employed; but the extent to which this increase in amount of action can be carried is limited. The rate in which this increase proceeds, and the influence upon that rate by the different conditions of strength of acid mixture, and relative distances of the plates, together with some other attendant phenomena, are examined in a succeeding section. My present purpose is to state the fact of this increase, and not to determine either its rate or its extent.

128. In the investigation now on hand, the amount of voltaic action obtained by a small couple of the same magnitude as those used in the last section, and having its plates at the relative distance of $\frac{1}{4}$ of an inch, is used as a standard of comparison.

129. The amount of action given by this couple being determined at this first position, in an acid mixture of uniform strength and dimensions, its copper plate is then removed to other and greater distances from the zinc, and the amounts given at those positions likewise determined. But the rate at which the amount decreases upon this increase in distance has already been determined by the experiments in the last section.

130. It is now proposed at these several distances, to increase the size of the copper plates till the amount of action obtained shall become equal to that yielded by the standard couple at the first or standard position; the immediate object being to determine the relation between the sizes of the plates required to produce this amount, and the distances at which they are required.

131. By way of illustration, let the distance to which the small copper plate is removed from the zinc be supposed to be 24 inches; and that the amount of action yielded there is (as under some conditions is the fact,) about three times less than that obtained at the first or standard position. Then by substituting larger copper plates in succession in the place of the small one, let it be determined by what size the amount becomes three times greater, or exactly equal to that given by the small plate at the first distance of $\frac{1}{4}$ of an inch.

132. But it is not yet proved that any addition whatever to the size of the copper plate, at the supposed position of 24 inches, will give an amount of action three times greater, or an amount equal to that required.

133. Before proceeding by an appeal to experiment to determine this point, it might be presumed that each successive addition to the size of the copper plate, whether at 24 inches distance, or at whatever other position the trial might be made,

would be followed by a corresponding increase in the action of the whole arrangement; and it might be presumed also, that inasmuch as the amount of action given by the standard plate is reduced from 3 to 1 by the removal from $\frac{1}{4}$ of an inch to 24 inches, but to a less extent in the intermediate positions, it would require a larger plate at 24 inches to restore the standard amount than at any of the intermediate positions, or in other words, that a copper plate progressively smaller would be required to restore that amount as we progressively advance to positions nearer to the zinc or standard position.

134. It will be of some advantage at this stage, before proceeding on to the more specific details, to give a general outline of the kind of results which have been obtained by the experiments entered upon to determine the points just suggested; only premising that the numbers and positions now used must be considered as illustrative merely, and not as actual representations of any one particular instance of the general phenomena and relations which have been thus detected; the whole of which, it will be found subsequently, are influenced by a variety of circumstances which could scarcely have been anticipated, and which it becomes the particular business of the inquiry to detect and estimate as it proceeds.

This general illustration is given in the hope that it may serve to simplify and more clearly define the nature of the somewhat complicated examinations upon which I am now entering, the precise numerical results of which are immediately to follow.

135. Let it be supposed that the trials are made in a mass of liquid contained in an oblong-shaped trough, similar to that used in the experiments of the last section.

136. Let the amount of action given at the distance of $\frac{1}{4}$ of an inch by small standard plates be supposed equal to 3, and that obtained at 24 inches off equal to 1; then, whilst at the latter position, let the small copper plate be removed, and others progressively larger be substituted for it in succession; and it will be found that by no increase whatever to the size of the copper plate, can the same amount of action be obtained at 24 inches, that was found by the small plate at the first or standard position. A certain increase in amount will be obtained, perhaps equal to 2, but in no case to 3, the amount required.

But let the same trials be now repeated at other positions nearer to the fixed zinc plate, say at the several positions reached by successive steps of 2 inches each, when the following unexpected results will be presented:—It will be found that neither can the required amount, 3, be obtained at any of

the several positions intermediate between the distance of 24 inches and 12 from the zinc, but that at 12 inches a certain-sized plate will give the amount sought for.

Proceeding on in the same manner still nearer to the zinc, the required amount will be obtained likewise at 10 inches and at 8, but only by plates larger than that yielding the same amount at 12; but at the distance of 6 inches again, as at 24, it cannot be obtained by any sized plate that the dimensions of the trough will allow to be tried; whilst at the remaining successive positions between 6 inches and the zinc or standard position, the required amount will be readily obtained by plates gradually diminishing in size, as the positions approach nearer to the zinc, till we again reach the first position of $\frac{1}{4}$ of an inch, and consequently obtain the amount by a plate equal in size to that first employed.

137. I proceed now to the details of the experiments by which these and the remaining results have been detected; and it will be borne in mind, that it is the relation between the magnitudes of the two copper-plates—the standard and the required plates—those by which the equal amount of action is given, and the distances at which they are respectively required, which is the result immediately sought for.

138. As a specific example of this kind of investigation, I select the following average instance, the details of which being given separately, will contribute to the better apprehension of the kind of results which are to be more fully embodied in the succeeding tables.

139. The small standard couple, with its copper plate at the relative distance of $\frac{1}{4}$ of an inch from the zinc, gives the usual measure of $\frac{1}{10}$ th of a cubic inch of hydrogen in 85 seconds; but removed to the distance of 10 inches, the time required is 160 seconds. Then, in the place of the smaller copper plate, and at the distance of 10 inches, are substituted other copper plates in succession, and progressively larger, when the time in which each yields the measure of hydrogen is as follows.

Table (No. 7).

| | At $\frac{1}{4}$ inch distance. | At 10 inches distance. |
|---|---------------------------------|------------------------------|
| Total surface in square inches of each copper-plate. | 2. | 2. 4. 8. 12. 18. 24. 32. 40. |
| Time in seconds in which each yields the equal measure of hydrogen. | 85' | 160" 120" 110" 90" 85" 80" |

140. Now it will be remembered always, that the fewer the

number of seconds required for the production of the measure of gas, the greater is the amount of voltaic action. In this instance we perceive that the amount is reduced from 85" to 160" by the removal from $\frac{1}{4}$ of an inch to 10 inches, and then at the latter distance, that the amount is again gradually increased by each addition to the size of the copper plate, till it again reaches to 85". It is still further increased by a further addition to the size of the plate; but I here purposely avoid carrying out the numbers beyond the one immediately sought for, namely, the required number, in this instance 85".

So soon as that amount is given, I cease to register any further results obtained at that particular position; for were the results pursued beyond this point, we should then be introduced to another series of changes, the consideration of which must be reserved for another section.

141. But besides that at some distances the action, by this addition to the plate, increases to and beyond the amount required, at others it never reaches that amount; and when the latter is the case, the trials by the larger plates are carried fully out to discover the number and the size making the nearest approach to the one required. And both the required number, when that is obtained, and where not obtained, that making the closest approach to it in amount, are distinguished by asterisks throughout the tables following.

142. Now it is seen in this example, that the standard or required amount 85" is obtained (when the plates are 10 inches apart,) by a copper plate having a total area of 18 square inches, whilst the same amount is given (at the distance of $\frac{1}{4}$ of an inch,) by one whose total area is only 2 square inches. It requires therefore, in this instance, that the copper plate of any couple should be nine times larger to produce at the distance of 10 inches, the same amount of action which is produced at $\frac{1}{4}$ of an inch. But this example gives the amount obtained for one position only, and under only one general condition of the whole arrangement, though the conditions under which the arrangement may be placed are various.

143. The first of the succeeding tables will contain the results obtained by testing the operations of an arrangement in this manner at each position, within the range of 12 inches from the zinc, whilst all its attendant conditions remain uniform; and such results being obtained fully for any one case, it is then required to find whether, if the acid mixture be altered in strength, these results will be altered likewise—in what manner and to what extent; and also, if possible, to discover whether there be any other causes influencing the operations of such arrangements than those originating in

the strength or density of the acid mixture, in the relative distances, and the relative dimensions of the two plates.

144. In accordance with this design, the second table will show the results of a like series of observations to the first, but made under a change in one of the conditions of the general arrangement, viz. in the strength of the acid mixture; whilst all other conditions remain the same as before.

Table (No. 8).

145. Showing the total superficial areas, in square inches, of the copper-plates; and the time, in seconds, in which each yields the $\frac{1}{10}$ th of a cubic inch of hydrogen at the different distances.

Area of zinc plate 1 square inch, specific gravity of acid mixture 1.013, the depth 6 inches. Trough the same as before. Temperature 55° F.

| Dimensions of
the copper-plates
in square inches. | | 2 | 4 | 8 | 12 | 18 | 24 | 32 | 40 | 50 | 60 | 72 |
|---|----|-------|------|------|------|-------|-------|------|------|------|------|------|
| Distance in inches
of the copper from
the zinc plate. | 4 | *170" | | | | | | | | | | |
| | 1 | 235 | *170 | | | | | | | | | |
| | 2 | 365 | 280 | 230" | 205" | *170" | | | | | | |
| | 4 | 360 | 300 | 270 | 225 | 215 | *205" | 270" | 245" | 245" | 245" | 220" |
| | 8 | 356 | 340 | 335 | 285 | 275 | 300 | 450 | *270 | 335 | 320 | 325 |
| | 10 | 390 | 320 | 300 | 285 | 275 | *270 | 385 | 345 | 335 | 335 | 335 |
| Time in seconds. | 12 | 450 | 370 | 335 | 300 | *345 | 385 | 675 | 460 | 450 | 400 | 425 |
| | | 475 | 380 | 335 | *300 | *300 | 450 | 670 | 405 | 450 | 400 | 405 |

146. It is shown by the results contained in this table, that the plates of the small standard couple, on being separated from one another through the several relative distances of $\frac{1}{4}$ of an inch and 12 inches, afford the same kind of results as have already been exhibited in tables Nos. 5 and 6.

147. These results are similar as regards the alternation in amount of action occurring at one particular position within that range, and in the progressive diminution of that action, which takes place as the distances increase from the first to the last position; that amount is equal to 170" at the distance of $\frac{1}{4}$ of an inch, and to 475" at 12 inches. These results are placed in the first perpendicular column of this table, underneath the number 2, representing the total extent of surface of the copper-plate by which they are yielded.

148. Then, on substituting at each distance, larger copper-plates in succession in the place of the standard one, we have the results thus afforded, shown in the horizontal lines of this table.

149. We see that at the distance of 1 inch, the standard amount 170" is obtained by a plate having a surface of 4 square inches, or one twice the size of that needed at a $\frac{1}{4}$ of an inch;

and that at 2 inches distance, the amount goes on increasing by each addition to the plate, till it reaches 170", which is given by a plate of 18 square inches, or one about 9 times larger than that needed at the first position, and 4 times larger than that needed at the second.

150. But beyond these distances, the amount is not obtained by any addition to the size of the plate. At 4 inches, the next position, occurs the singular appearance of the greatest amount of action, 205", which occurs throughout the range of plates at that distance, being produced by a plate small in size compared with many employed in it. This is yielded by a plate of 24 square inches, whilst others larger and ranging between 24 and 72 square inches give a less amount.

151. The same phenomenon occurs at the remaining distances, but to a different extent, and by different sized plates in each. These particular numbers in each column, wherever the amount sought for is not obtained, are considered as those making the nearest approach to that amount; and constituting as they do valuable data for future reasoning, they are those distinguished in the table by a star, and by the dotted lines in the subjoined diagram.

Table (No. 9).

152. The same as before in all respects, except the strength of the acid mixture, which is 1.090 specific gravity.

| Surface of the
copper-plate in
square inches. | | 2 | 4 | 8 | 12 | 18 | 24 | 32 | 40 | 50 | 60 | 72 |
|---|----|-------|-----|------|-----|-----|------|-----|------|-----|------|-----|
| Distance in inches
of the copper from
the fixed zinc plate. | 1 | * 55" | | | | | | | | | | |
| | 2 | 100 | 60" | *55" | | | | | | | | |
| | 4 | 130 | 80 | 75 | 65" | 60" | *55" | | | | | |
| | 6 | 135 | 90 | 85 | 70 | 75 | 65 | 60" | *55" | | | |
| | 8 | 135 | 105 | 90 | 85 | 70 | 75 | 75 | 70 | 65" | *60" | 70" |
| | 10 | 135 | 95 | 75 | 60 | 65 | *60 | 75 | 65 | 65 | 70 | 75 |
| | 12 | 150 | 85 | 80 | 75 | 75 | 75 | 85 | *65 | 70 | 70 | 70 |
| Time in seconds. | | 155 | 100 | 90 | 85 | 70 | *60 | 75 | 65 | 65 | 70 | 70 |

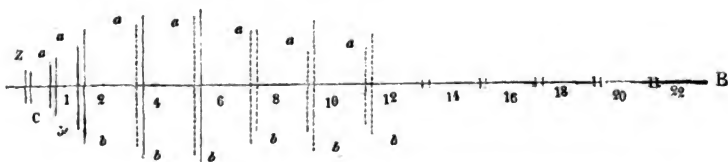
153. We perceive that throughout this table the same general phenomena are presented as in the former one, differing only in degree; an effect which must be attributed to the difference in the strength of the acid mixture, that being the only condition of the general arrangement in which any change has been made.

154. The standard amount of action 55", is here regained at the 3 several positions 1, 2, and 4 inches distance; whereas in the other case, it was regained only at the two first. And the plates so required to reproduce this standard amount are of a larger size comparatively, but appear to increase by a regular progression, as in the former instance.

155. In the former case that progression is 2, 4, and 18; in the latter, 2, 8, 24, and 40. There is a sufficient degree of regularity in each of these rates of increase, without the necessity of making more than a reasonable allowance for the imperfections of actual experiment, to indicate that the phenomena in question are guided, as in every other class of voltaic action, by the influence of some fixed law, to which these numbers must be considered as making but a mere approximation.

156. Beyond the distances at which the standard amount is required in this table, we see, as in the former one, that a certain maximum amount of action is obtained in each column, and by different sized plates in each. At 6 inches, the next position, a maximum amount of 60" is given by a plate of 60 square inches, whereas at the position following, or at 8 inches, the same amount is yielded by a plate of only 24 square inches, and so on; the operations, after we have passed a particular position, suffering a series of peculiar changes or alternations, exactly similar in their general character to those observed in the fifth table.

Fig. 2.



157. For the sake of still more clearly exhibiting the results which I have here more particularly in view, I will transfer them from the table to the annexed diagram (Fig. 2), in which the horizontal line A B represents the length of the mass of liquid in which the plates are acting, being supposed to pass through its centre, and the perpendicular line C D its depth. At Z C are the standard plates at their first position, and the numbers 1, 2, 4, &c. on the horizontal line, show the different distances to which the copper plates are successively removed and tried. The plain lines passing at right angles through this horizontal line, represent the particular copper-plates (in the exact linear measure of their squares), which at each position yield the required amount of action; and the dotted lines represent also the exact dimensions of those plates which yield, at the respective positions, the amount nearest to the one sought for, when that amount itself is not obtained. The

whole figure shows the exact relative proportions of the plates, the exact distances and dimensions of the mass of liquid, on a scale reduced from those actually employed.

158. The lines *a, a, a*, represent the copper-plates found by the experiments contained in the first table (No. 8), and those marked *b, b, b*, the plates from the second table (No. 9). It is almost needless to remark, that the plates, though shown separately in the figure for the sake of the comparison, must be considered as having occupied precisely the same position in actual experiment.

[To be continued.]

XXIV. *Researches on Heat. Third Series.* § 1. *On the unequally Polarizable Nature of different Kinds of Heat.* § 2. *On the Depolarization of Heat.* § 3. *On the Refrangibility of Heat.* By JAMES D. FORBES, Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

[Continued from p. 113 and concluded.]

§ 3. *On the Refrangibility of Heat.*

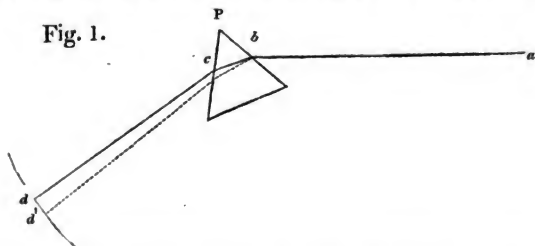
SINCE the admirable discovery by M. Melloni of the power of rock-salt to transmit and refract heat of every kind, one of the most obvious and important questions (formerly intractable) of which it seemed to offer the means of solution, was the accurate determination of the refrangibility of heat from various sources, luminous or non-luminous. Such a determination is of the first consequence to the formation of a just theory of heat, and a detection of the subtle bond by which it is connected with the comparatively familiar modifications of light.

Such experiments have not been wanting. M. Melloni, in his second memoir on radiant heat, in the *Annales de Chimie* for April 1834, has described the apparatus which he employed, and which is figured in Plate III. of that volume*. It consists of a thermo-electric pile, constructed of a single vertical row of elements, so as to be exposed to a very narrow beam of heat. It was made to move on a sector of a circle, at whose centre was placed a prism, by which the beam of heat was refracted from its primitive direction *ab* into that *cd*, (see next page), and therefore produced a maximum effect on the galvanometer when the pile was at *d*. The other parts maintaining the same positions, it is evident that the pile must be moved into the position *d'*, if the source of

[* A translation of Melloni's second memoir on radiant heat will be found in the *Scientific Memoirs*, vol. i. p. 39.—EDIT.]

heat be now one yielding rays of greater refrangibility. Although the radius of the circular arc was (if I understand the

Fig. 1.



account rightly) eleven inches, but little deviation of position was required for heat from different sources; and M. Melloni admits that, whilst his experiment *indicates* the difference of refrangibility, it is inadequate to *measure* it.

There are many reasons why such a form of apparatus must be rejected for accurate observations. I will mention only the impossibility of obtaining a beam of heat which shall preserve the same breadth at different distances from its source (of course, supposing the rays rendered as parallel as possible by refraction through a rock-salt lens), arising, 1. from the angular magnitude of the source; 2. from the scattered reflection and refraction at the surfaces of the lens and prism; 3. from the want of homogeneity of the ray. On all these accounts, the beam must have acquired a very sensible breadth at the distance of the pile, and consequently the effect of heat must be perceptible, and even nearly uniform, through a certain space. I may also add from experience, that the difficulty of varying the arrangement of an experiment, so as to get a maximum heating effect at the pile, is so considerable, that no delicate result can be deduced from the merely tentative procedure. Finally, the smallness of the variation of refrangibility seems to require some more critical method of ascertaining its measure. On all these grounds, it seemed to me desirable to discover a method in some degree less open to objection.

The phenomenon of total reflection, successfully employed by Dr. Wollaston in the measurement of refractive indices in the case of light*, presents the advantage of being (theoretically at least) abrupt in its action, the transition from partial to total reflection being (with the necessary exception arising from the want of homogeneity) an instantaneous change, amounting in the case of light to many times the intensity of

* Phil. Trans. 1802.

the smaller effect. It seemed reasonable to expect, that an apparatus constructed on the principle of determining the critical angle of total reflection of heat from different sources within a prism, would afford much more definite information as to the refrangibility of heat than any other method. After much consideration, an apparatus of the following kind was adopted.

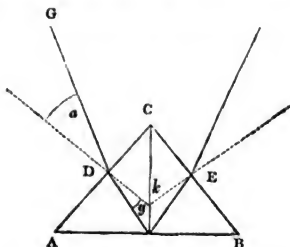
It is fundamentally composed of a jointed frame, resembling a box exactly square, ten inches in the side, without top or bottom, and having hinges at every angle, so that it may be formed into a lozenge of any degree of obliquity. This is seen in Plate IV. fig. 1, and marked A B. By an arrangement presently to be described, the rays of heat are made to pass parallel to the edge ac of one of the sides of the box, and to fall upon the prism P, whence, after undergoing reflection (total or partial) at the posterior surface of the prism, they proceed parallel to the line ad , and fall upon the sentient extremity of the pile at p . Now, in order that this course may be taken by the reflected rays, it is necessary that, supposing the prism to be an isosceles one, the posterior reflecting surface $a' b'$, fig. 2, should form equal angles with the incident and reflected rays ce and fd . It was to effect this that the arrangement of the jointed lozenge was adopted. The prism P (fig. 1.) rests on a column O, moveable round the line of junction of the sides C and D of the lozenge. The column O has connected with it a tail-piece of brass $a E$ passing through the diagonal of the frame, and preserved constantly in that position by a slit parallel to its length, through which passes a clamping screw b , serving at once to maintain this constancy of direction, to secure the form of the moveable lozenge, and by means of an index pointing to a graduated scale of inches reckoned from a , along $a E$, to determine the length of the diagonal ab at any moment, and consequently the angles of the lozenge.

A little consideration of this mechanical arrangement, will show how it is adapted to the end in view. The rays from a source of heat S, rendered parallel by the lens of rock-salt L, fall upon the prism P, and after undergoing two refractions and one reflection, they fall upon the sentient surface of the pile p . This will always take place so long as the posterior surface of the prism forms equal angles with the lines ac , ad , which will be secured by making it truly perpendicular to the tail-piece $a E$, by which it is guided, and which of course always bisects the angle cad . Now, it is evident that, whilst the angle cad remains small, the reflection will continue *partial*, but that as the diagonal ab is shortened, a point will

be reached when *total* reflection abruptly commences, which ought to be indicated by a *saltus* in the movement of the galvanometer connected with the pile. This critical angle will be soonest attained for rays of greatest refrangibility, and the calculation of the refractive index of the prism is reduced to a simply mathematical problem.

The following is the problem to be solved, viz.: *A ray of light G D (fig. 2.) falls upon the surface A C of a prism, which has the angles at A and B equal; it falls upon the surface A B at the critical angle of total reflection; required the index of refraction (μ) of the prism, the angle of incidence (α) being given.*

Fig. 2.



An investigation of little difficulty gives the following result.

$$\mu = \sqrt{1 + \left(\frac{\sin \alpha + \sin \frac{1}{2} I}{\cos \frac{1}{2} I} \right)^2}.$$

I had a rock-salt prism constructed, so that the incidence on the first surface might be nearly vertical at the critical angle of total reflection, so as to avoid as much as possible any error arising from imperfections of the surface, or want of absolute equality of the angles at A and B; and likewise, that within the limits of the experiment, the loss of heat by reflection at the two surfaces might be nearly unaltered, as it is believed to be almost constant at incidences tolerably nearly perpendicular*. This prism, constructed for me by Mr. John Adie, had two angles of 40° and one of 100° ; and so accurately was it made, that (satisfying myself with a careful measurement by the common goniometer, extreme nicety being unimportant) the angles appeared to be true to those quantities within a few minutes of a degree.

By a reference to Plate IV., fig. 1, it will now be understood that the required arrangement is of this kind. The heat diverging from the source S, is converted into an ap-

* See Melloni on the Reflection of Heat, *Annales de Chimie*, Dec. 1835.

proximately parallel beam by the lens *L*. It then passes through a diaphragm *T*, placed on one or other side of the prism (it does not much matter which, as the beam which arrives at the pile is always much wider than the second diaphragm *t*, placed there to admit only the central rays arriving parallel to the line *a c*). The use of this diaphragm is, that a narrow enough pencil of rays may be employed, to be independent of the variable breadth under which the surface of the prism is presented to the incident beam. The usual dimension of this diaphragm was one inch in breadth and one and a quarter in height, but in some instances its breadth was reduced to three-eighths of an inch.

The pile *p* has its funnel-shaped orifice closed by a screen with a vertical slit, an inch wide, in the direction of its axis. But there is a peculiarity in the arrangement of the pile very essential to the success of these experiments, where the pile itself is moveable, which I must not omit to mention. Its exposure to currents of air would render the observations, when the pile cannot be entirely inclosed by a box or screen, very capricious in its action. I therefore adapted to the end, bearing the conical reflector, an adjustable wooden tube *r*, containing a rock-salt lens, which still further increased its sensibility, and totally protected it from aërial currents.

The abruptness of the effect of transition from partial to total reflection is far from being so complete as might be wished; and this is easier accounted for than remedied. It arises mainly from the magnitude of the source of heat, the consequent want of parallelism of the refracted rays, the scattering of these rays in consequence of the imperfect polish of the surfaces, the unequal intensity of the rays in different parts of the section of the cylinder, and lastly, owing to the want of homogeneity of the rays of heat from any source, which the method would serve to measure, were the other imperfections removed, just as in the course of the total reflection of light, prismatic colours are successively presented.

My first rude attempts showed all this very clearly. As the diagonal *a b* of the lozenge (fig. 1) shortened, total reflection obviously succeeded to partial, and the change was not only very great, but near one point very rapid. The point where the most rapid increase took place, is obviously that where the greater proportion of the incident rays underwent total reflection, and might therefore be taken as a mean representation of the quality of the heat. Still the change was too gradual to enable one by mere inspection to determine this point with accuracy, and I speedily resolved to take the sure but laborious method of ascertaining at a number of points

intermediate between total and partial reflection the intensities of the reflected heat, and by constructing a curve having measures of the diagonal of the lozenge (a function of the angle of incidence) for abscissæ, and intensities for ordinates, I endeavoured to discover graphically for what value of the former the measure of the latter increased most rapidly, in other words, where the tangent made the greatest angle with the axis, or where was the point of contrary flexure of the curve.

Plate IV. fig. 2, may represent such a curve. I have found that when the diagonal of the lozenge was 14.5 inches, the reflection was in all cases nearly total, or the galvanometer was little affected by any increase of the angle of incidence. This effect, measured by the vertical line A B, was denoted by 100. When the diagonal was increased to 15.0, the effect was reduced, we shall suppose, to 90, expounded by the line C D, at 15.5 by E F, and so forth. An interpolating curve drawn through the points so fixed, would have its greatest inclination to the axis A X, when, for a given variation of the diagonal, the decrement of the intensity was a maximum, in other words, at the determining angle for the predominating part of the heat used. Such a point of contrary flexure would therefore determine the *mean* index of refraction of the given kind of heat by the aid of the formula above investigated, whilst the form of the curve would lead to some conjecture at least, respecting the distribution of heat of the more or less refrangible kinds in the given ray. Heat of low refrangibility being the last to be totally reflected, would cause the curve to droop fastest near the extremity B, the more refrangible rays would be cut off at the other end I of the curve.

I lost no time in verifying the general truth of the principle, and also of the received doctrines respecting heat, by examining the *quality* of the heat which reached the pile at different stages of total reflection. If, as M. Melloni first rendered probable, heat of *low* temperature is *least* refrangible, and *vice versa*; and further, if it be admitted that such heat passes most difficultly through such substances as glass, it follows, that after total reflection has proceeded a certain way, so that the more refrangible, and therefore more transmissible, rays have suffered total reflection, whilst the remaining rays constituting the primitive beam continue to be refracted, the heat thus reflected will be more copiously transmitted by glass, than when it came direct from the source. This conjecture was precisely verified.

Subsequent experiment still more fully confirmed this result, and by showing that during the whole progress from partial

to total reflection, the specific quality of the heat changes, gave countenance to the view that the gradation is in a great measure owing to the want of homogeneity of the heat, and that the figure of the curve becomes (as we have said) a real test of the composition of a ray.

At the inferior limit of the curve, or when partial reflection takes place, all kinds of heat are equally reflected (in the case of light, the light is white), just as at the superior limit, or after total reflection is complete, the beam has exactly the same relative composition as before. In the intermediate stages the composition is perpetually varying. The first rays totally reflected (and combining with the scattered and partially reflected rays) are the more refrangible, or those more easily transmitted by glass. At a certain point a maximum proportion of these enter into the reflected beam. As the angle of incidence becomes greater, more and more of the less refrangible rays enter into the composition of the reflected heat, which at last possesses the same qualities as at first. This is well illustrated by the following early experiment which I made on the proportion of the reflected rays transmitted by a plate of glass .06 inch thick, at different stages of reflection (7th February 1838).

| Diagonal
<i>a d</i> , Fig. 1
in Inches. | Deviations of Galvanometer. | | Ratio. | REMARKS. |
|---|-----------------------------|-----------|----------|----------------------------|
| | Glass. | No Glass. | | |
| 14.5 | 8.3 | 13.75 | 60 : 100 | Total reflection complete. |
| 15.0 | 7.85 | 12.65 | 62 : 100 | |
| 15.25 | 7.1 | 10.9 | 65 : 100 | |
| 15.5 | 5.5 | 7.85 | 70 : 100 | |
| 15.75 | 3.4 | 5.1 | 67 : 100 | |
| 16.0 | 2.3 | 3.75 | 61 : 100 | |
| 16.5 | 1.45 | 2.3 | 63 : 100 | Partial reflection. |

In the following experiments on the law of the transition from partial to total reflection, the arrangement was that shown in Plate IV. fig. 1, and described (with the adjustments) in pages 182—184; the centre of the pile *p* was 13 inches from the prism *P*, and the distance of the source of heat *S* from *P* was 12 inches; that a diaphragm *T*, whose aperture was 1 inch by $1\frac{1}{4}$, was placed in the path of the ray usually between *P* and *L* near *P*; the aperture of the pile was contracted to a breadth of one inch, whose centre was exactly in the line *ad*; and only that part of the prism was employed which was free from flaws capable of producing total reflection.

The diagonal of the lozenge frame was varied from 14.5 inches up to 16.5 or 17.0, about eight observations of the in-

tensity of reflected light being made at intervals. The series was then frequently *reversed*, and the mean results of the going and returning series taken to allow for any change which might have occurred in the intensity of the source. In all cases an observation of verification was made and such change allowed for. The dynamical effect on the galvanometer was observed and noted.

In reducing the observations the following plan was adopted. The intensity corresponding to the diagonal 14·5 inches being assumed = 100, the other intensities were reduced relatively to it, and projected, as explained in art. 60. By this means different series of observations became at once comparable with each other, and the beauty and regularity of the curves thus formed, and the almost perfect identity of those obtained on different days, and with different adjustments, give a degree of confidence in the results which is extremely satisfactory. When from the nature of the heat the effect was very small (as in the case of alum being interposed, or the source being of low temperature), I have endeavoured to supply the deficiency by multiplying observations, and the uniformity of the curves thus obtained has been the test of my success. Where this test has failed (as in the attempt to work with heat of 212°), I have suppressed the results.

Example.—Dark Hot Brass. March 31, 1838.

| Measure of
Diagonal <i>a b</i> ,
Plate xiii. Fig. 1.
in Inches. | Galvanometer-Needle. | | Excess. | Ratio to Result
with Diagonal
= 14·5. |
|--|----------------------|-----------|---------|---|
| | Stands at | Swings to | | |
| 14·5 | A 0·1 | A 11·0 | 10·9 | 100 : 100 |
| 15·0 | 0·0 | 9·3 | 9·3 | 85 : 100 |
| 15·25 | 0·15 | 8·0 | 7·85 | 72 : 100 |
| 15·5 | 0·15 | 6·2 | 6·05 | 55 : 100 |
| — | 0·3 | 6·3 | 6·0 | |
| 15·75 | 0·25 | 4·6 | 4·35 | 40 : 100 |
| 16·0 | 0·15 | 3·15 | 3·0 | 28 : 100 |
| 16·25 | 0·2 | 2·25 | 2·05 | 19 : 100 |
| 16·5 | 0·15 | 1·9 | 1·75 | 16 : 100 |
| 14·5 | 0·05 | 10·8 | 10·75 | |

After the observations made as now described have been projected in the form shown, Plate IV. fig. 2, the diagonal corresponding to the maximum rate of decrease of the intensity was determined, for the purpose of deducing the index of refraction. The following enumerations of the kinds of heat employed, and the results derived from the several pro-

jections, will give a just idea of the confidence due to the results.

Sources of Heat.—(1.) The direct rays of the Locatelli lamp. A slightly concave reflector was employed. (2.) The same lamp, with a reflector having the form of a portion of a sphere concentric with the wick; the heat transmitted through *alum*. (3.) Heat from the same source transmitted by *window glass* .06 inch thick. (4.) Heat from the same transmitted by *opaque black glass* (through which the disk of the unclouded sun is just visible). (5.) Heat from the same transmitted through *dark coloured mica*, by which direct sunlight is absolutely stopped. This singular substance I long sought for in vain, it is unknown to many practical mineralogists; it transmits green light at small thicknesses, when thicker its colour is hair-brown. By reflected light its colour is between green and black. (6.) Heat from incandescent platinum. (7.) The same sifted by *window-glass* as above. (8.) The same sifted by *opaque mica*. (9.) Heat from dark brass about 700° . This is obtained from a nearly cylindrical cover of smoked brass placed over the flame of a spirit-lamp, so as entirely to conceal it, and which gives remarkably good results, without increasing considerably the angular breadth of the source (which is greatly to be avoided when a lens is used). It is in fact not much greater in size than the helical coil of platinum wire used in (6). (10.) The same, sifted by clear *mica* .0044 inch thick. (11.) Heat from a crucible of mercury about 450° . The crucible was about 2 inches in the side, smoked externally, and heated by a spirit-lamp. The temperature of the mercury which it contained (covered with sand) was noted at each observation by means of an inserted thermometer.

The results were the following:

| Source of Heat. | α δ , or diagonal
corresponding to the
point of contrary flexure. | μ , or Index of Refraction
for mean rays computed
by the Formula, page 183. |
|--------------------------------|---|---|
| Locatelli, direct..... | 15.49 | 1.571 |
| —— with alum ... | 15.76 | 1.598 |
| —— window-glass | 15.65 | 1.587 |
| —— opaque glass | 15.71 | 1.593 |
| —— mica | 15.61 | 1.583 |
| Incandescent platinum | 15.50 | 1.572 |
| Ditto with glass..... | 15.66 | 1.588 |
| —— opaque mica | 15.62 | 1.584 |
| Brass at 700° | 15.45 | 1.568 |
| Ditto with clear mica | 15.55 | 1.577 |
| Mercury at 450° | 15.50 | 1.572 |
| Mean luminous rays... | 15.8 | 1.602 |

The preceding results give values of μ all too high; that for light is known by other methods to be only 1·53 or 1·54. This arises from the increasing intensity of the partially reflected light with the obliquity of the incident ray which makes the apparent transition from partial to total reflection too rapid, and consequently gives the index too great. The true indices may be approximately found by diminishing these numbers by ·07; but the *relative* results are the most important.

The results which we have obtained apply, it must be recollected, only to the *predominant* kind of heat in any source, and that we have as yet got no information respecting the *composition* of a ray and the amount of dispersion.

It is very easy to see that were the mathematical conditions of the experiment (p. 183) fulfilled, we should be led to an exact analysis of heat, more perfect far than we have any prospect of obtaining in the case of light, considering the difficulty of applying the photometer to coloured light. Were the curve in Plate IV. fig. 2, solely representative of the progress of reflection due to the heterogeneity of the rays, the increment of intensity between any diagonal E and another C, or D f would denote the proportion of the entire heat incident, which lies between the limits of refrangibility assigned by the diagonal. Thus an entire ray would be decomposed into parcels of known proportions, between given intervals of refrangibility. The case is considerably different. Though the points of contrary flexure agree remarkably well, as we have seen, the curves are in some cases much more flattened than in others, where the source of heat is the same; owing probably to the greater parallelism of the rays at one time than at another, depending on the distance of the source of heat from the lens.

We can, therefore, in this way form but an imperfect idea of the comparative homogeneity of the different kinds of heat. Such comparisons can only be made advantageously by comparing the results obtained in immediate succession from one and the same source with interposed screens of different qualities, as in the comparison which we instituted between heat direct from Locatelli's lamp, and that transmitted by glass, (p. 186).

The facts respecting refrangibility, which may now be considered as ascertained, serve to render our ideas much more precise in several respects. For instance, (1.) the range of *mean* refractive indices for heat is small, all the modifications which we have considered lying within a range of ·04, or between 1·51 and 1·55 nearly, which is little more than

the commonly assigned dispersion of light, which for rock-salt, is between the limits 1.54 and 1.57 nearly. This, however, is for *extreme* rays of light, which can hardly be said of heat; the extremes of dispersion are certainly much wider apart. (2.) The *mean* refractive index of *direct* rays from different sources varies surprisingly little. In fact the differences for *direct* rays of heat from the Locatelli-lamp, incandescent platinum, and from a crucible heated to 450°, seem almost insensible, or within the limits of error of experiment. It is to be recollected, however, that this is compatible with the utmost variety in the *composition* of each. (3.) The effect of interposed screens in modifying the transmitted heat is very remarkable. These, so far as I have tried them, invariably *raise* the index of refraction, (alum, glass, opaque glass, and opaque mica for the Locatelli-lamp; glass and opaque mica for incandescent platinum, and clear mica for dark heat). This is the case even with those substances which suppress light altogether, and which therefore cannot be considered to do more than detach the heat of considerable refrangibility from the light which usually accompanies it, not as stopping the most refrangible rays and admitting the passage of those of lower temperature. Probably no substance acts in this way, though some (as black glass and mica, as the experiments of Melloni indicate) may probably absorb the heat spectrum at *both* extremities. It is probably to this source that we must attribute the very small fraction of heat transmitted by the black glass I used, being only that constituting the rays of the higher degrees of refrangibility, all those of *low* and *mean*, and also of the *highest*, degrees of refrangibility being probably absorbed. (4.) With respect to the homogeneity of different kinds of heat, we can deduce nothing *certain* from the forms of the curves. They confirm, however, a view which I have long entertained, that heat from non-luminous sources is more homogeneous than any other. I argued this partly on the ground stated in p. 111 of this volume, and still more from the uniformity of results which I have in all classes of experiments obtained from dark heat, which often more than made up for the narrower range of the thermal effect, and which showed that the discrepancies observed in other cases were due not so much to errors of observation, as to unavoidable changes in the character of the heat, (p. 100). This result is the more probable from the size of the source of heat necessarily used in the crucible experiments (p. 188), which tends to render the passage from partial to total reflection more gradual, and thus to flatten the curve. To the same cause may also probably be attributed the somewhat greater

index of mean refraction obtained for heat from this source than that of dark heat of higher temperature.

The following method might perhaps be used with success for obtaining more exact data respecting the refrangibility, and especially the *dispersion*, of heat, than that just described pretends to give. It must insure a beam of parallel rays of heat of sufficient intensity and uniform in every part of its section. A small point of heat placed behind a lens (or two or three lenses to diminish aberration) is the most obvious plan. But the intensity would be inadequate. I would, therefore, propose a platinum wire, heated by one of Mr. Daniell's constant voltaic batteries, placed behind a refracting semicylinder of rock-salt*. The central rays should be alone employed, and the prism for total reflection should be high and narrow as well as the aperture of the pile. It is possible that in this case the transition from partial to total reflection would be so rapid as to make the error arising from the varying intensity of partial reflection inconsiderable. By changing the force of the battery, heat of all temperatures might be employed in succession. The numerical analysis of the heat spectrum would then take place as described in p. 189.

Conclusion.—My object in these, as in former researches, has not been to group experiments of mere curiosity indiscriminately selected, but to present a basis for a proper theory of heat. Without some such end in view I should have thought the time and labour spent on these experiments in some degree misapplied. Mere numerical results, though ultimately of the highest consequence to science, should never form the exclusive object of the philosopher. I trust to have shown that though many of the conclusions in this paper are based upon quantitative results, these have not been the ultimate aim of the inquiry.

The mutual bearing of the three sections of this paper, and of all upon what (from analogy to physical optics) we may call *physical thermotics*, is now evident. (1.) In the first section we have minutely discussed a point apparently perhaps of minor importance, namely, the unequally polarizable nature of the rays of heat. The importance of the doctrine lies in this: that the common theory of undulation recognises no such variation, nor *perhaps* does it exist in the case of light (I know, however, of no decisive experiments on this point), with the exception of the small effect due to the difference of refrangibility. Now, having proved in the third section that this difference of mean refrangibility is from most sources

* Such a one I have had executed.

very small, which yet differ widely in their polarizability, we infer that that explanation is probably inadequate, and that we must look for a mechanical theory of heat differing in some particulars from that of light.

(2.) This latter conclusion is further confirmed by the results of the second section, in which is deduced, from the singularly accordant results of wholly distinct series of experiments with heat from those distinct sources, that the phenomena of depolarization differ surprisingly, *numerically speaking*, from those of light, whilst in their general character they are entirely similar. The results at which we have arrived oblige us to admit, *either* that the length of a wave of heat is several times greater than that of a wave of light, *or* that the velocities of the ordinary and extraordinary ray in doubly refracting crystals are totally different from those of light; *or else* a combination of these hypotheses. Now, of the two first alternatives, we are bound at present, I think, to prefer the latter, since we know nothing of the phenomena of double refraction but from this experiment; whilst the subsequent experiments on the refractive index, would, according to the prevalent theory of dispersion, seem to show that the mean length of a wave of heat cannot differ very materially from one of light. This amounts to admitting that the doubly refractive energy is more feeble for heat than for light; in other words, that a greater thickness of a crystal is required to produce a given effect. The second and third sections also confirm one another in this respect, that the uniformity of the results of depolarization with heat from different sources, and also of the refrangibility, would both be highly improbable did the length of a wave materially differ in those instances.

(3.) Of the results of the third section, I have already spoken at sufficient length (p. 189). The mean index of refraction for all kinds of heat tried is *less* than for light;—it ranges within narrow limits;—when the heat from different sources is unmodified by transmission through diathermant bodies, these limits are very narrow indeed;—the measure of dispersion is considerable but unascertained, and opens a fair field for experiment;—dispersion is probably least for sources of low temperature.

Such are the chief data for speculation afforded by the experimental results contained in this paper:—too imperfect perhaps in themselves to form the basis of a mechanical theory of heat, yet such I hope as may be considered to be fit contributions towards its construction at a future period.

Edinburgh, April 16, 1838.

XXV. *Researches on Suppuration.* By GEORGE GULLIVER, Esq., Assistant Surgeon to the Royal Regiment of Horse Guards.

SECT. I.—*On the frequent presence and on the effects of Pus in the Blood, in diseases attended by Inflammation and Suppuration.**

IN the prosecution of an inquiry in which I have been long engaged concerning Inflammation and Suppuration, I soon perceived the necessity of instituting a careful examination of the blood in these affections, and particularly in the different forms of inflammatory fever and hectic.

The result has been the detection of pus in the blood in almost every instance in which there was either extensive suppuration, or great inflammatory swelling without a visible deposition of pus in any of the textures of the body: and the contamination of the blood by pus appears to me to be the proximate cause of the sympathetic inflammatory, sympathetic typhoid, and hectic fevers. Since the writings of Dr. Lee, Mr. Lawrence, Mr. Arnott, of MM. Velpeau, Dance, and others, the profession has become familiar with cases in which pus has been found in the veins, particularly after surgical operations and in uterine phlebitis; but although the humoral pathology has of late years begun to assume some of its ancient importance, I am not aware that any writer has attempted to demonstrate the dependence of the fevers under consideration on the presence of pus in the blood.

Previous to a brief notice of some of the experiments and observations from which the results have been drawn, it may be proper to mention the means by which I have detected pus in the blood. The examination was very simple,—partly chemical, and partly by the aid of the microscope. Those who are acquainted with the minute constitution of the animal fluids are aware of the rapid and energetic action of water on the blood-corpuscles: now the globules of pus undergo no change after having been long kept in water; accordingly, if the suspected blood be mixed with this fluid, the blood corpuscles will soon become invisible, and any globules of pus that may be present will subside to the bottom of the vessel, and may be easily seen, and their characters determined, with a good microscope. Ammonia instantly renders the blood-corpuscle invisible, while that of pus is acted on but slowly by the alkali; and the different action of acetic acid on pus

* Read before the Royal Society, June 14, 1838; and now communicated by the Author.

and blood is equally remarkable. Hence I have employed these agents advantageously in conjunction with the other means; and I have also seen pus-globules in the blood, though rarely, without any preparation. With water, however, the examination is most easy, simple, and satisfactory, if the observer be thoroughly familiar with the microscopic characters of the fluids under examination. A good instrument, nevertheless, is necessary; and the admirable deep object glass of Mr. Ross is the one I have principally employed. It is hardly necessary to add, that chyle-globules are not likely to be mistaken for those of pus, since, independently of other distinctions, the medium diameter of the latter is at least $\frac{1}{800}$ ths of an inch, which is above twice that of the former.

Exp. 1. A weak solution of corrosive sublimate was injected into the subcutaneous cellular tissue of a dog's thigh; great swelling of the limb took place, and he died forty-five hours after the injury. A good deal of serum mixed with fibrine was found in the cellular tissue of the thigh, but there was no purulent deposit.

Several pus-globules were detected in some blood obtained from the right ventricle of this dog's heart.

Exp. 2. A large dog had both his tibiæ injured by some operations connected with necrosis; great swelling of the limbs, with violent fever, succeeded, and he died forty-three hours subsequently.

A large quantity of fibrine was found effused into the cellular tissue of the extremities, mixed, in one of them, with a very scanty proportion of purulent matter.

In some blood, obtained from the vena cava, numerous globules of pus were observed.

Exp. 3. An irritating fluid was injected into the peritoneum of a dog; he had great thirst, refused food, and died the third day after the operation.

A large quantity of coagulated lymph and sanguinolent serum with some pus was found in the belly.

In some blood obtained from the inferior cava vein many globules of pus were seen.

Exp. 4. Two ounces of pus were injected into the left pleura of a dog, and very carefully confined there; he was thirsty and feverish for fifty-five hours after the operation, when he was killed.

An ounce of fluid, almost entirely serum, was found in the pleura, and some fibrinous exudation on the membrane.

Blood from the heart, as well as from the vena cava was examined, and found to contain several pus globules.

Exp. 5. Four ounces and five drams of pus were injected into the peritoneum of a dog, and the wound carefully closed; he died thirty-seven hours after the injury.

There were only nine drams of a sero-sanguinolent fluid found in the peritoneum, and a considerable quantity of coagulated lymph on the membrane.

Pus was detected in the blood.

Exp. 6. Half a dram of pus, mixed with half an ounce of water, was gradually injected into the crural vein of a dog.

Some fever followed, and he refused solid food for two days, but recovered at the end of a week.

The same quantity of pus was soon afterwards injected into the other crural vein, when similar symptoms were produced, and he perfectly recovered in a few days.

Exp. 7. Six drams of pus having been injected into the crural vein of another dog, he was not much affected at first, but in a few hours became very weak, was stupid, thirsty, and refused his food. After thirty hours he took but little notice of surrounding objects, his respiration was hurried, and he died thirty-six hours after the operation. In the blood of the inferior cava some pus globules were readily detected.

Case 1. A girl died of confluent small pox on the ninth day of the disease. There was great swelling of the integuments.

In the blood of the right ventricle numerous pus-globules were found.

Case 2. A woman had confluent small pox, uncomplicated with erysipelas or inflammation of the viscera.

On the eighth day of the disease some blood was drawn from a vein in the arm: several pus-globules were found in this blood.

Case 3. A male child, æt. 15 months, died on the ninth day of small-pox. Only a few pustules appeared, and these were imperfectly developed: there was considerable swelling in the face, slighter in other parts.

At the post-mortem examination, it was observed that a small quantity of a white opaque fluid might be squeezed from the cut surfaces of the lymphatic glands of the neck and groin: this fluid had the microscopic and chemical characters of pus.

In some blood obtained from the right ventricle and from the inferior cava vein, pus was detected.

Case 4. In a woman who died of puerperal peritonitis, the peritoneum contained a large quantity of coagulated lymph, serum, and purulent matter.

Pus was detected in the blood obtained from the right ventricle of the heart.

Case 5. James Green, æt. 27. was admitted into hospital with an ulcer of the leg. Seven days afterwards, the limb began to swell, and there was hardness in the femoral vein, with some redness in the course of the absorbents on the inner side of the thigh. The swelling of the limb increased gradually; he had first pain in the head, thirst, and quick pulse; then purging, pain in one wrist, with restlessness, incoherency of speech, and offensive breath; finally, low muttering delirium, accelerated respiration, and coma preceded his death, which took place on the twelfth day after his admission into hospital.

At the post-mortem examination, the large veins of the limb were found to be occluded throughout by firm clots of blood, mixed with pus and coagulated lymph, and the lining membrane of the femoral vein was in many places of a red colour, and coated with fibrine. In the iliac vein no such signs of inflammation appeared, although there was a large coagulum of blood, which had lost its red colour, containing in its centre a small quantity of matter resembling pus. Several purulent deposits presented in the sheath of the femoral vessels, and in the intermuscular cellular substance.

The matter resembling pus in the clot of the iliac vein had neither the chemical nor microscopical characters of that fluid.

In some blood obtained for examination from the right ventricle and from the vena cava, numerous globules of pus were found.

Case 6. James Hawke, æt. 22, had a superficial wound of the tibia, followed quickly by considerable pain and swelling. There was a very scanty deposit of pus in the subcutaneous cellular tissue. The swelling of the limb increased and extended rapidly, the integuments becoming discoloured, and the slight suppuration ceasing. His dissolution was preceded by subsultus, collapsed face, accelerated breathing, hiccough, and coma.

The swelling of the limb was found to be produced by effusion of fibrine and sanguinolent serum. A few pus-globules were found in the blood obtained from the vena cava.

Case 7. M. Jackson, æt. 42, had erysipelas of the face, which decreased, and was succeeded by jaundice and effusion

into the pleura. He became listless and low, with accelerated respiration, and died six days after the appearance of the erysipelas.

An ounce of turbid serum, with a little purulent matter, was found in the right pleura, and eight ounces of sanguinolent serum in the left.

Some blood was obtained for examination from the larger veins, and found to be greatly contaminated with pus.

Case 8. Sergeant Dunn, æt. 29, had profuse suppuration between the muscles and beneath the integuments of the thigh; he died, after some weeks' suffering, exhausted by hectic.

The purulent matter was extremely offensive, putrefying with great rapidity, and sometimes coagulating spontaneously, when set aside for a short time. It was poor in true pus-globules, but contained a large quantity of flaky fibrinous matter, to which its opacity was chiefly owing. Many pus-globules were found in the blood obtained from the right ventricle.

Case 9. Wm. MacLean, æt. 19, died of pulmonary consumption. In his lungs were several vomicæ, containing pus and softened tubercular matter.

In the blood obtained from the vena cava and right ventricle, many pus-globules were found.

Case 10. A man had irritative fever, in the Marylebone Infirmary, consequent on a large abscess behind the trochanter femoris.

An ounce of blood was drawn by cupping from the neighbouring sound parts, and some pus was detected in this blood.

Case 11. An officer's charger died with vomicæ and tubercles in the lungs, and sero-purulent fluid in one pleura. Some time before his death his respiration and circulation were much accelerated.

The vomicæ contained pus mixed with gangrenous sanies.

In the blood obtained from the vena cava inferior pus was detected.

The preceding instances by no means comprehend the whole number in which I have found pus in the blood. In the detail I have rather been anxious to give examples of interesting varieties, than to increase the number by needless repetitions.

It is satisfactory to add, that the observations of Dr. Davy tend to confirm the accuracy of those which I have just related. He detected pus in the blood of consumptive patients, after my general results had been submitted to him, but be-

fore I had turned my attention to the state of the blood in phthisis. He has lately informed me that he has found pus in the blood in seventeen instances after death, in sixteen of which there was declared suppuration, and in one none could be detected: in the latter, the patient died of acute inflammatory disease.

Before considering the conclusions to be deduced from the preceding observations, it may be proper to advert briefly to the nature and use of suppuration, although I shall have occasion to bring forward the evidence on matters of opinion in a more systematic form in a future part of these researches.

Since the microscopic observations of Mr. Hunter, Sir Everard Home, and Mr. Bauer, the opinion has often been expressed in this country, that the globules of pus are nothing but those of blood, modified by the inflammatory process. I believe Sir Astley Cooper and the late Dr. Young came long ago to this conclusion. Finally, on the continent, M. Gendrin, without much regard to the observations of English pathologists, adopts precisely the same theory, supported indeed by a series of very ingenious experiments, which have been generally considered conclusive on this subject.

I have repeated the experiments of M. Gendrin with great care, and although I see no reason to dissent from that part of his conclusion already stated as having been long since advanced in this country, I have not been able to observe the phenomena related in his work. It seems not improbable that M. Gendrin was influenced by the erroneous views of M. Milne Edwards as to the globular structure of fibrine; for M. Gendrin states in one place that pus is but a modification of fibrine, although in others he informs us that it is a transformation of the blood-corpuscles that constitutes suppuration. By cauterizing the web of a frog's foot under the microscope, or by elevating on the polished blade of a lancet a film of the edge of a wound previously made in the part, he assures us how easy it is to see the blood-particles gradually transformed into those of pus. I regret to say that I have not been able to succeed in this observation, because I found, after repeated trials, that I could not by any means induce suppuration in batrachian reptiles.

With regard to the conversion of clots of fibrine into pus, some experiments to be adduced in another section of this inquiry, render it extremely probable that the matter often found in the centre of such clots in the heart and great vessels, is nothing more than softened fibrine; and which, although it resembles pus in some particulars, presents neither the chemical nor the microscopical character of that fluid. I have

seen nothing like pus-globules in the softened fibrinous clots of the heart; and the rounded particles which sometimes occur in softened coagula of veins are probably the remains of blood-corpuscles. The conversion of the latter into those of pus is extremely probable, and it is equally probable that this change may take place either in the capillaries or out of them. In the former case, after the stagnation of the blood in these vessels which preceded the suppurative process, as the clot softened and the pus became mature, it would be carried into the circulation, and hence its presence in the blood independently of wounds or abscesses.

In instances of idiopathic or traumatic phlebitis, the manner in which the pus may become mixed with the blood is obvious enough. There is a class of cases to which the latter appellation is commonly applied, which are probably not examples of inflamed veins. They seem rather to be of an opposite nature; for I have seen large veins, which had been divided many days before death, containing purulent fluid, although their inner surfaces presented no marks of inflammation; and the total failure of this process in them would seem to have left open their wounds, so as to favour the entrance of pus into them from the neighbouring parts: and this consideration would involve an important point of practice.

It might be asked if, on a suppurating surface, the pus-globules, considerably larger than those of blood, escape from the capillaries, how comes it that the latter particles do not escape as well? To which it may be answered, that the discharge of the pus-globules is preceded by the coagulation of the blood in these vessels; and that their closure, where there is a breach of continuity, is provided for by the formation of the clot keeping pace with its decomposition during the suppurative process; and the blood corpuscle, reduced in size by being broken down, or by losing its external part, may escape, and still become enlarged out of the vessels from the addition of new matter, till it assumes the character of a true pus-globule: hence its larger and more unequal size and irregular surface than the blood corpuscle.

I think my experiments will render it probable that suppuration is a sort of proximate analysis of the blood. As the effused fibrine produces swelling, or is attracted to the contiguous tissue for the reparation of injury, the blood corpuscles, altered by stagnation, become useless, and are discharged in the shape of pus, as waste from the system. Suppuration, therefore, would appear to be a physiological rather than a pathological phenomenon—pus being an excrementitious discharge—a part of the blood which has become effete and

noxious during the reparative process, whether this process may have been employed in limiting the extent of an abscess or in healing breaches of continuity. If, however, there should be a formation of pus in the capillaries, in consequence of the stagnation and coagulation of their contents, this pus might be mixed in large quantities with the blood in cases where there was no declared suppuration, as in some of the examples brought forward in this paper.

With regard to the correct observation of Müller, that the smaller capillaries have only the diameter of a blood corpuscle, I shall on a future occasion show, from the result of experiments, that these vessels become sufficiently enlarged during inflammation to contain a row of pus-globules.

If it should be remarked that pus is often formed without any obvious addition of fibrine to the neighbouring parts, it should be recollected that this is not a healthy, but a diseased form of suppuration; and the distinction and explanation are not difficult. In the formation of the unhealthy pus in question, the fibrine is broken down, mixed, and excreted with the pus; and hence the flaky, curdy appearance of such matter, its proneness to putrefaction, and the cases cited by some authors as instances of suppuration without inflammation, and the old term, "badly matured matter." Independently of the paucity of true pus-globules in this kind of discharge, with the abundance of flaky particles, its tendency to putrefaction would afford strong proof of its containing fibrine but little changed in its composition; for of all the animal fluids, pus is probably that which resists putrefaction with the greatest pertinacity. The eighth case, that of Dunn, is but one among many that I could cite in illustration of these observations.

It remains to deduce the conclusions from the experiments and observations related in this paper.

The term suppurative fever is not new, and its signification is probably now extended; for it seems to be an appropriate one for the different forms of constitutional disturbance under consideration. If the presence of pus in the blood and the fever in these cases be not related as cause and effect, the coincidence would appear to be no less interesting than remarkable.

What a field of inquiry this view opens to us! Henceforth, whenever a patient is affected with inflammatory fever, or that low typhoid state which is so generally a forerunner of death, as a consequence of traumatic or idiopathic inflammation, the state of the blood will present an interesting subject of investigation. And this is not merely a matter of curiosity; for the question will arise, whether, in the treatment

of such cases, it would not be advantageous to produce suppuration as soon as possible on the surface of the body, so as to establish a drain by which the blood might be deprived of the offending matter. It may be asked also, whether the benefit so often effected by blisters, setons, and issues, in certain internal inflammations,—or by incisions, which cause suppuration, in inflammatory affections of the integuments, be not explicable by this theory? It is well known that in cases of traumatic or idiopathic inflammation, attended with great swelling and febrile excitement, the establishment of suppuration in the part is generally a favourable symptom, the separation of the pus from the blood being a sort of crisis to the symptomatic fever. In small-pox, it is a popular belief that “the striking in,” as it is termed, or suppression of the pustules, is a bad symptom; and this is so far true, that the worst cases of this disease are those in which there is great swelling of the integuments without the due formation of pus in the usual situation. In every instance in which I have examined it, I found pus in the blood of patients affected with small-pox.

In the fourth and fifth experiments the pus which was injected into the serous sacs would appear to have been absorbed. A more careful inquiry, however, would be requisite to warrant this conclusion; for in some experiments made by Dr. Davy, the quantity of matter injected seemed to be increased; and I have since made an experiment with the same result.

The absorption of pus being the cause of hectic fever is an old hypothesis, which the detection of pus in the blood in cases of chronic abscess and in pulmonary consumption might be supposed to confirm. It does not seem necessary, however, to assign two causes for one effect. When pus in large quantities is incessantly forming in the capillaries, it is easy to imagine how it may become mixed with the blood.

I have related instances of pus in the blood, independently of suppuration out of the vessels: this fact appears to be of some importance, for it must be inferred that the pus was not absorbed, but formed in the blood.

If it be objected to some of the foregoing views, that pus and extravasated blood are often absorbed without any ill effects, and that no constitutional disturbance may ensue after inflammation and the consequent effusion of fibrine—it may be remarked, first, that pus and blood are probably absorbed in a modified state; and secondly, that a small quantity of pus, like other poisons, gradually added to the circulation may not be productive of bad symptoms. The sixth and seventh experiments may be cited in illustration. It is pro-

bable that the degree and type of the fever induced by the presence of pus in the blood may be found to depend on the extent to which it may be contaminated.

Of the inflammatory, hectic, and low typhoid fever, it seems hardly necessary to observe, that they appear to be all comprehended under the common designation of constitutional irritation in the interesting work of Mr. Travers, which I had not read till my attention was directed to it by Mr. Liston after this paper was written. 'Under the term typhoid, I have included that grave form of fever in which the vital powers sink rapidly, as I believe, from somewhat sudden and extensive mixture of pus with the blood, as sometimes occurs after operations on veins, or amputations, or even independently of wounds. The patient seldom complains of much pain; he has, among other symptoms, dilated nostril, flushed face, encrusted tongue and teeth, restlessness, small quick pulse, cold clammy sweats, offensive breath, hiccough, subsultus, stupor.

I cannot conclude this paper without expressing a hope that it will lead to a still more careful and extensive examination of the blood in various diseases than has hitherto been attempted. The microscope may become as important an instrument to the pathologist, and even to the medical practitioner, as the stethoscope. If my results should be confirmed, it is hardly too much to expect that some important discovery, particularly in diagnosis, may be made by a patient investigation of the blood in many malignant diseases, such as cancer: it is not long since the urinous fever, as it is called, was found to depend on the accumulation of urea in the blood.

XXVI. *Instructions for the Qualitative Analysis of Soluble Salts.* By JOHN JOSEPH GRIFFIN, Author of "*Chemical Recreations.*"*

[The salts are supposed to be in a state of purity, and each to contain one of the twenty-seven metals, and one of the fourteen acids, that are named in the tables.]

DISSOLVE the salt in water, and apply to separate small portions of the resulting solution contained in test tubes, or in conical test glasses, a few drops of the test solutions that are named at the head of each column in the tables, commencing with those on the left hand.

Pay no attention to the precipitates that are not mentioned in the tables.

* Communicated by the Author.

PRECIPITANTS FOR METALS IN SALTS.

| Solutions to be Neutral. | | | | Solutions to be Acid. | METALS indicated by the Precipitates. |
|--------------------------|----------------------|--|---|-----------------------------------|--|
| Carbonate of Soda. | Ammonia. | Potash. | Red Prussiate of Potash. | Sulphuretted Hydrogen Gas. | |
| None
None
None | | | | | 1 Potassium.
2 Sodium.
3 Ammonium. |
| | None
None
None | | | | 4 Barium.
5 Strontium.
6 Calcium. |
| | | White
White
White
White
White
All 5 are insoluble in excess | Brown
Blue
None | Yellow
Black | 7 Manganese.
8 Iron, protosalts
9 Magnesium.
10 Cadmium.
11 Bismuth. |
| | | White
White
White
White
White
All 6 are soluble in excess | Yellow-Red
White | None
Black
Yellow
Orange | 12 Zinc.
13 Tin, protosalts.
14 Aluminum.
15 Lead.
16 Tin, persalts.
17 Antimony. |
| | | Black,
see Gold No. 25. | Red-Brown | | 18 Mercury, its protosalts. |
| | | Blue,
If boiled, Red.
Blue,
If boiled, Black. | | | 19 Cobalt.
20 Copper. |
| | | Green
Green
Green | Yellow-Green
None
Light-Blue | | 21 Nickel.
22 Chromium.
23 Iron, persalts and protosalts mixed. |
| | | Yellow,

Yellow, sometimes slight and Black. | Yellow-Red,
But none from the Perchloride.
None | | 24 Mercury, its persalts.
25 Gold. |
| | | Brown
Brown | None
Brown | | 26 Iron, persalts.
27 Silver. |

SUPPLEMENTARY TESTS.

For Nos. 1, 2, 3, of the metals.

Salts of *Ammonia* give the odour of ammonia when warmed with a few drops of a solution of potash.

Salts of *Soda* held in a dry state in a small blue flame colour it yellow.

Salts of *Potash* do neither.

For Nos. 4, 5, 6, of the metals.

Salts of *Barytes* give precipitates with solutions both of the yellow and the red chromate of potash.

Salts of *Strontian* with the yellow chromate alone.

Salts of *Lime* with neither.

| PRECIPITANTS FOR ACIDS IN SALTS. | | | | | |
|----------------------------------|--|--------------------|------------------|--|--------------------------------------|
| Nitrate of Barytes. | | Nitrate of Silver. | Nitrate of Lead. | Chloride of Calcium. | SALTS indicated by the Precipitates. |
| None | | None | | | 1 Nitrates. |
| None | | None | | | 2 Chlorates. |
| None | | White | | | 3 Chlorides. |
| None | | | Yellow | | 4 Iodides. |
| None | | | White | | 5 Arsenites. |
| None | | Black | | | 6 Sulphurets. |
| White | All 5 soluble in Nitric Acid, without Effervescence. | None | | | 7 Fluorides. |
| White | | Yellow | | | 8 Phosphates. |
| White | | Brown | | | 9 Arseniates. |
| White | | | | White Sol. in water
White Insol. in water | 10 Borates. |
| White | | | | | 11 Oxalates. |
| White | Soluble in Acids with Effervescence | | | | 12 Carbonates. |
| White | Insoluble in Acids | | | | 13 Sulphates. |
| Yellow | | | | | 14 Chromates. |

SUPPLEMENTARY TESTS.

For Nos. 1, 2, of the acids.

Dry *Nitrates* heated with dry bisulphate of potash in a glass tube, produce a red gas.

Dry *Chlorates* do not.

Easy method of applying Sulphuretted Hydrogen Gas as a Test.—Take a test tube, an inch wide and six inches long. Put into it half a grain of sulphuret of iron, or sulphuret of antimony, and two or three drops of muriatic acid. Insert into the mouth of the tube a slip of white paper, three inches

long and half an inch wide, wetted with the solution under examination previously acidified. Allow a portion of the paper to project beyond the tube and secure it by inserting a cork. Warm the bottom of the tube over a spirit-lamp. The action of the gas on the solution is manifested by the change produced in the colour of the paper. As in Nos. 10 and 11, Table I. you have only to distinguish between black and yellow, and in Nos. 14—17, between no change, black, yellow, and orange, all of which are easily perceived upon white paper, this method of testing accomplishes the object equally as well as the more operose and disagreeable methods in common use.

Glasgow, May 1, 1838.

XXVII. *Justification of the Contact Theory of Galvanism.*

By G. TH. FECHNER.*

THE present memoir was already complete before I received Pfaff's late work "*Revision der Lehre vom Galvano-Voltaismus*," which has in general the same object in view. As I find that Pfaff's experiments refer rather to other points of the subject than to my own, I think that this paper will not have been rendered superfluous by that work, but that the one may serve as the completion of the other.

The chief arguments which can with any appearance of weight be brought to bear against the theory of contact, and in favour of the chemical theory, have lately been brought together by the zealous supporter of the latter, De la Rive, in a separate work "*Recherches sur la Cause de l'Electr. Volt.* 1836." I shall chiefly refer to this, especially since the partisans of the chemical theory rest principally on De la Rive's experiments. Faraday's late experiments militate not so much against the voltaic theory as (at least apparently so) against a theory which seeks for the origin of electricity solely in the contact of *metals* with each other, on which subject I shall at the conclusion have occasion to add a few words. Moreover they have been, together with the experiments of Karsten, which may be considered in the same light, treated of in detail by Pfaff; I can therefore pass them over at present. I have myself my own opinion on the subject, which I would rather produce at another time and with experiments, than at present with words. As to the following experiments, I have frequently repeated and varied them, in order to derive the expression from facts, not from accidents, from

* From Poggendorff's *Annalen*, vol. xlii. p. 481. Translated by Mr. W. Francis.

which, I confess, I do not consider De la Rive's experiments to be entirely free.

I. *Facts which refer to the unclosed circuit.*

1. I have given in these *Annals* (vol. xli. p. 225) the means of proving with certainty the not inconsiderable amount of electricity which two heterogeneous metals acquire by contact in an insulated state. The supporters of the chemical theory are accustomed either to deny the certainty of these experiments, a denial which will henceforth be rendered impossible by the application of the methods here stated, or to derive the electricity thus originated from friction, pressure, or the chemical influence of the air and its moisture. It is to be shown that the result cannot depend on these. I must however remark previously, that it appears to me somewhat strange to allow to friction and pressure, which after all are only particular modifications of contact, the property of exciting electricity, independent of chemical action, while it is so obstinately denied to the simple contact. Nor has it even once been tried to derive thermo-electricity from chemical action: the agency of contact is here undoubted.

That the result, in the above-mentioned experiments, cannot depend on friction, has already been shown in my memoir in these *Annals*, vol. xli. p. 235, under No. 4.*. That it does not depend on pressure, is evident partly for this reason; that wherever the contact is completely effected, which naturally cannot take place entirely without pressure, an increase of the pressure does not indicate any more evident influence on the power of the shocks; and partly, that, according to the experience of Becquerel (so very extensive on this subject) all bodies are capable of having electricity excited in them by pressure, with the single exception however of the case, in which both bodies pressed together are good conductors, in which it at least cannot be demonstrated, undoubtedly for the same reason which hinders the demonstration of the electricity produced through the friction of two good conductors. But that chemical action cannot be the cause of the result follows already indirectly for this reason; that in this case the condensation of the electricities at the surfaces of contact would be entirely inexplicable, which on the other hand is very easily explained according to the voltaic theory. In fact, it is self-evident, that, if a force exists in the contact of both plates, which can separate and transfer to the respective plates the electricities, notwithstanding their attraction for each other, it must also be capable of keeping them separate in a degree proportional to the intensity of that force.

* The statement here alluded to will be given in the sequel of this translation. EDIT.

It would then in *this* respect occupy the place of the layer of varnish of the condenser. What however could, according to the view that the developed electricity depends on chemical action (which can least of all take place at the surfaces of contact), hinder the transference of the condensed opposed electricities into one another, and allow of the condensation even taking place? Neither can it be a layer of air or of oxide placed between the two, since this must for the same reason oppose itself to the separation of the two electricities; further, such a layer must then also appear as a hindrance to the transfer to the electrometer, where the attraction for the opposed electricity does not even support the transition. In the mean time De la Rive has mentioned several experiments, which were to prove in a direct manner, that contact, without contemporaneous chemical action, was incapable of developing electricity. We will at present follow him in these experiments.

2. Already, some time ago, De la Rive had published some experiments, which he now again brings in support of his view (*Recherch.*, p. 57), according to which the signs of electricity, produced by two plates in contact in common air, no longer appear in a vacuum and in dried air. Nevertheless it is evident from the manner in which he performed these experiments, that their negative result is rather owing to his having connected the condenser made use of with the earth, by means of perfectly dried wood: now every one may convince himself that even in performing this experiment in common air the condenser under these circumstances refuses its services. Besides, Pfaff has already previously refuted these experiments experimentally, and with regard to further observations on this point I may refer to p. 20 of his work.

3. De la Rive, previously, in another place, and lately in his *Recherch.*, p. 60, has made the following experiment of importance.

“A piece of potassium or sodium was fixed in a solid manner by one of its ends to a platinum forceps, while the other extremity was held by means of a wooden forceps, or what was better, an ivory one. If, after having well brightened it, it is surrounded by very pure oil of naphtha, [*steinöl*] and the condenser be touched with the end of the platinum forceps, no electrical sign is observable; while if the naphtha oil is taken off and none remain adhering to the metal, this is observed to oxidate rapidly by the contact of the air, and the electricity indicated by the electroscope is of the most lively kind. The condenser is scarcely necessary to render it perceptible. If sometimes some indications of electricity are obtained when the potassium

or sodium is in the oil of naphtha, then a small quantity of humidity has been introduced into the liquid, which had remained adhering to the surfaces of the two metals, and which exercises on them a chemical action which it is easy to recognise. Immersed in azote and in hydrogen the two metals still gave rise to a development of electricity proceeding from the action exerted upon them either by the gases, or by the aqueous vapour, from which it is impossible entirely to free them (?); and in proof of this chemical action, we see their surfaces lose their metallic brightness and become tarnished very much as would have taken place in the air."

The result which I would draw from a repetition of this experiment is this; that in the form adopted by De la Rive, it is altogether unfit to give a proof of the one or the other view, because potassium brought into connexion with the earth by means of *dried* wood, does not allow us to perceive either *out of or in petroleum*, [*steinöl*] any development of electricity, which, according to both views, is explicable from the property of the condenser; but brought into connexion with it, by means of *moist* wood, it immediately produces, both in and out of the petroleum, powerful action, which the supporter of the chemical view can explain from the chemical action produced through the moisture, the supporter of the voltaic theory, from the increased conducting power of the wood; of the influence of which in this case sufficient evidence is contained in the following details. The experiment, however, may be, in fact, so varied, that it becomes decisive, and in this form it speaks, as will be seen, *against* the chemical theory. The following is the detail of the experiments.

A potassium ball was furnished at two opposite points with fresh surfaces of section: into one surface was inserted the end of a platinum wire, into the other the end of an air-dried bar of wood, so that the ball was situated between both; now the highly sensible condenser (at times brass, at times copper) which was connected with the electroscope with a dry pile described by me in these *Annals*, was brought into contact with the platinum, while the wood was held in the hand. I could perceive by the well-known manipulation with the condenser either no, or very inconsiderable, traces of electricity. I then substituted for the wood a cut chip of a quill, and obtained the same result. If on the contrary, I held the potassium ball in the hand, and brought the condenser into contact with the platinum, I obtained a divergence of the gold-leaf, of remarkable strength*, without comparison much more

* This was even the case when the potassium ball was not cleansed from the adherent petroleum; and I cannot in the least from my experi-

powerful than we are accustomed to obtain with zinc; this is therefore well adapted to serve as an experiment of demonstration in lectures. The same was the case if I held the bar of wood after slightly moistening it; and in the latter case, it was quite indifferent for the production of the effect, whether the ball was immersed in petroleum or not. I must therefore suppose, that De la Rive, since he obtained a result out of the petroleum, but none when the ball was in it, produced the conducting connexion with the earth through wood or ivory only within the petroleum, but when out of the petroleum by a more humid conductor; otherwise there would be a direct contradiction between his and my observations. One may easily believe, since this contradiction did not escape me, that I frequently repeated and varied the form of this experiment, in order to convince myself that on repeating it in various ways, right would remain on my side.

It remained now to observe, whether the absence of divergence by the use of air-dried wood was owing to the want of conducting power of the wood, or to the want of chemical action: for it could not indeed be denied, that, if the potassium ball was held with the hands or by means of a moist bar of wood, their moisture must act chemically upon it, which also, when the experiment is performed under petroleum, may easily be recognised by the bubbles of gas rising from the point of insertion of the slightly moistened wooden bar. The following experiment, more direct than all which De la Rive cites in support of his views, proves quite decidedly that the bad conducting power of the air-dried wood alone at least *suffices* to explain the negative result. If I moistened one half of the bar which stood in connexion with the potassium, and also the point of insertion in the potassium, just as in the former experiments in which I obtained a result, but held the wood during the contact of the condenser with the platinum by the half which remained air-dried, I obtained the former negative result; nay, this was even then the case, if while thus manipulating I moistened the potassium during the contact of the condenser with acidulated water, so that a kind of explosive, chemical action took place. By this then the insufficient conducting power of air-dried wood for such experiments is sufficiently proved, and every one may easily, and even without potassium, convince himself by substituting zinc for that metal, when the phenomena take place in quite

ments concur in the view of Ohm and Pfaff, that an isolated intermediate layer of petroleum acted a part in De la Rive's experiments.

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the same manner if the same arrangement be observed. We see therefore that De la Rive's experiment, in petroleum, according to his account of it, could not succeed, either according to the chemical theory, or according to the voltaic theory, and that when it is all taken together, he proves nothing whatever. I succeeded however in varying these experiments so that an argument may be drawn from them *against* the chemical theory.

From the extreme vivacity with which the divergence took place when the potassium attached to the condenser by means of a platinum wire or even by direct contact (for in effect the platinum is in this case quite unnecessary) stood in connection with the earth by a moist conductor, and from the excessive sensitiveness of the electrometer with a dry pile which I employed, it appeared to me not improbable, that even without a condenser, by the contact of a negative metal with potassium, a divergence would take place. If the brass point from which the gold-leaf was suspended was brought into contact either directly, or even with the interposition of an air-dried strip of paper or linen, which appeared to conduct much better than air-dried wood, with the platinum wire of the potassium ball, whilst the latter was held immediately with the hand or by a slightly moistened slip of wood, there might be perceived, when the gold-leaf had a certain stability, a very weak, but undeniable, negative electrical divergence; and also a positive electrical one, if by reversing the combination I held with my fingers the platinum wire, and brought the electrometer with the intermediation of the air-dried inter-conductor, or even only a layer of oxide which had formed on the potassium, into contact with that metal. From the numerous and varied experiments, sometimes bringing the electrometer into contact with insulated, then uninsulated platinum, *without the agency of potassium*, I became convinced, that in fact no divergence of the electrometer would be produced by such contacts; the contact of the potassium was necessary; similar counter-experiments have not been neglected in the following.

From the distinctness of the signs obtained (which as yet do not afford any objection against the chemical theory) it appeared to me not impossible, even by the entire insulation of the potassium, to render signs of electricity perceptible, and thus entirely to do away with the influence of moisture. Under these circumstances, the condenser, for known reasons, cannot be made use of; on the other hand, it is true that this difficulty stood opposed even to the result of the application of an

electrometrical apparatus constructed on different principles, namely, that the intensity of the electricity of two heterogeneous plates in contact is in the inverse ratio of the magnitude of their surfaces or magnitude of conduction; consequently, if the superficies of the electrometer be considerably larger than that of the potassium, and moreover the former be not well insulated, no result can be expected. In effect I could no longer obtain those signs of electricity which I had previously recognised with such complete certainty, if I held the potassium in a dry insulating forceps and touched my electrometer with the platinum wire of the potassium. I completely succeeded, however, even with this fine experiment in the following manner. I had an electrometer purposely made with the smallest surface possible, consisting solely of a very thin and short brass wire, which, as the axis of a surrounding gum lac cylinder, traversed the perforated bottom of an inverted drinking-glass, and from which within the glass was suspended between the pole plates of the pile of the electrometer a very small gold-leaf, $2\frac{1}{2}$ inches long, while the electricity could be transferred to the prominent end of the brass without the glass. Into the potassium ball was inserted a thin platinum wire, as short as the convenience of transfer of the electricity allowed, and the ball itself, for the purpose of increasing its surface, was pressed between two copper plates which had been soaked in petroleum, as smooth as was possible without cutting the potassium ball with the platinum wire. Thus the entire electrometer might have been somewhat about double the size of the surfaces of the potassium*.

With this arrangement I now repeated the former experiments in the air, holding at times the potassium with the insulating forceps, at times the platinum, while the other metal was each time brought into connexion with the electroscope. If clear, opposite divergence of the gold-leaf could in this case be observed with the most complete certainty; for this arrangement, if the combination of potassium and platinum was not insulated, proved to be much more sensitive than the one previously employed, and gave without the condenser the most evident divergences, (opposite by the opposite arrangement,) which might easily be increased up to the striking of the gold-leaf against the pole plates, if these were merely separated so far from one another ($7\frac{1}{2}$ lin.) as was necessary for the stability of the gold-leaf. I now placed the potassium

* I endeavoured to unite several balls of potassium by pressure, but did not succeed, either from the petroleum which remained attached to them, or from the oxide which immediately formed at the surfaces of the fresh sections.

disc with the upwards bent wire, proceeding from it, in a small glass, covered it with petroleum up to about half an inch high, and discharged the platinum wire which projected from the petroleum (and which nowhere touched the glass) on to the electrometer, while I held the glass in my hand. *The divergence to the side, which indicates the negative electricity, followed in this case quite as constantly, evidently, and certainly as if the potassium had been insulated in the air.* I have already mentioned that the counter experiments in this case were not neglected.

The following modes appear to me still to offer themselves for the explanation of the result of this experiment according to the chemical theory:

a. Some moisture was brought together with the potassium into the petroleum, the chemical action of which caused the result.

b. The petroleum was perhaps adulterated and still capable of acting chemically on the potassium.

With respect to *a.* this objection has at first sight the appearance of some weight, since it is true that when the potassium is brought from the air into the petroleum, we observe during some time a few gas bubbles rise up from the potassium, which undoubtedly are produced by the chemical action of the adhering moisture. But this development of gas soon ceases; and long after this had *entirely* disappeared, twenty-four hours afterwards, during which time the potassium continually remained immersed in the petroleum, (I did not observe it for more than twenty-four hours afterwards) I have obtained the electrical signs in the petroleum *of quite the same force*, as during the development of gas and even in the air; so that this objection thus completely falls to the ground.

As to the objection *b.* the petroleum employed was that in which the potassium had already been preserved for many years without becoming anything more than deprived of its lustre at its surfaces; while if the petroleum had contained any oxygen, this must necessarily have produced a gradual destruction of the potassium; and even if the oil had in the beginning contained any oxygen, this would certainly have been consumed during the long preservation of the potassium in it, so that I conceive this second objection also to be completely refuted.

4. An experiment on which De la Rive appears to lay particular stress, since he refers to it in several places, is the following (*Recherch.*, p. 67.):

“ I took two zinc plates exactly similar with regard to their dimensions to the brass plates of an ordinary condenser;

I soldered to each a brass knob (*bout de laiton*); I covered their inner surface with a thin layer of lac varnish, so that they might perform the office of the plates of a condenser; I further entirely covered the exterior surface of one of the zinc plates with a layer of the same varnish, so that this plate was not in immediate contact with the air at any of its points. Various experiments were made, forming the condenser sometimes with the two zinc plates, sometimes with one of them only and with a brass plate. When the zinc plate which I employed was that the entire surface of which was covered with varnish, I constantly obtained electrical signs much less strong than in employing that one of which the exterior surface was entirely uncovered and exposed to the immediate contact of the air. Presuming that the electricity, very feeble it is true, which was developed with the varnished zinc plate, arose from the circumstance that the layer with which it was covered was too thin to intercept completely all chemical action of the air and of the humidity, I successively increased the thickness of this layer, and I succeeded in rendering it such that the plate ceased to give any electrical signs. What moreover proves that it is to the action, even through the varnish, which the moist air could exercise upon the surface of the zinc, that the production of the electricity was owing, was that I observed after a short time a commencement of oxidation take place on this surface. We see, therefore, that when a zinc plate, by means of a layer of varnish, is entirely protected from the action of the air or of those agents which might exert a chemical action upon it, it does not become electric in its contact with a brass knob. Still more; this inactive apparatus conducts itself as a homogeneous plate of brass. Thus on touching the brass knob which was soldered to the surface of the zinc, with the copper of a heterogeneous plate, of which I held the zinc in my hand, I succeeded in charging it with negative electricity. Or, according to the contact theory, the two coppers being in immediate contact, and placed between the two zincs soldered to them, viz. that of the condenser and that which I held in my hand, no result ought to have taken place. We might obtain a result of the same kind, perhaps, still more striking, by uniting for the purpose of forming the conductor the two plates of zinc and making the two brass knobs soldered to them communicate with one another. In the theory of contact, the opposition of these two pairs, perfectly similar, ought to have neutralized all kind of action; however, the experiment showed that the zinc plate, the naked surface of which was exposed to the air, became charged with positive electricity, while that of which the

surface was covered with a thick layer of varnish became negative, just as a brass plate would have become in the same case."

These experiments struck me in a very high degree, and I have repeated them quite according to the method mentioned by De la Rive, with all the care which I considered to be due to experiments, which, accordingly as their result might turn out, would really testify for or against the chemical theory. But the issue was simply this; that the results, after the varnishing over of the zinc condenser, to which was soldered a copper knob, and after the varnish had become completely dry, were not perceptibly distinguishable from those which I obtained by over varnishing with the same condenser, although the layer of lac varnish with which I had covered by often repeated coatings the entire non-condensing surfaces of the zinc condenser (while the condensing surface could only retain their thin coating of varnish, as was the case in De la Rive's experiments according to an express statement of his in another place) was laid on excessively thick, and with the most anxious care that no point of the zinc might be left free. To go a step further still than De la Rive, I fixed, in order to lay aside the objection which perhaps might be raised respecting the chemical action of the air upon the copper, a platinum wire to the copper point, and now varnished over even the whole copper point, so that platinum alone remained free. If the platinum was now touched with the finger or with a slip of paper which had been moistened in distilled water, the zinc condensers became quite as well charged with positive electricity as if it had not been varnished. Besides we are already in possession of some experiments of Becquerel and Peltier (vide Becquerel's *Traité*, ii. p. 139) which have given a result similar to my own. However, in Becquerel's a small part of the zinc instead of being varnished was merely covered by glass, which De la Rive does not consider as being sufficient. In Peltier's experiments this circumstance was avoided; but the action of his apparatus, which was arranged in a different manner, I have not been able fully to comprehend. But also in Pfaff, I find (vide his work, p. 22.) that he had repeated De la Rive's experiment quite in accordance with his own statement and had always observed the same action of the zinc condensers with varnish as without varnish.

Moreover I have succeeded in deciding affirmatively the question, whether varnished zinc in connection with electro-negative metals develops electricity in a way by far more simple than by means of the condenser.

By the application of an electrometer, quite similar to the one described under No. 3, except that it has a longer gold-

leaf ($4\frac{1}{2}$ Parisian inches), and instead of a brass point passing through the lac, one of gold, I obtain with a zinc plate to which a platinum wire is fastened, a constant divergence of the gold-leaf to the right or left, very small, it is true, but yet subject to no deception, accordingly as I connected the one or the other metal with the gold wire, while I held the other in the hand; only by the connexion of the zinc with the gold wire the interposition of a moist conductor is, for reasons easily understood, necessary; but not so, if I connected the platinum with it. For the latter case only I have employed unvarnished zinc, as also coated with a thick layer of varnish (so that even the point of connexion of the zinc with the platinum was carefully covered with it), and in both cases I obtained results not differing in any perceptible degree (namely, a remarkable equally strong negative divergence). The size of the zinc plate was such that the surface of the electrometer was inconsiderable in comparison with it. For the success of this experiment the pole plates must be placed as close as is compatible with the stability of the gold-leaf.

5. De la Rive makes of importance (*Recherch.*, p. 66) that Becquerel by means of sensitive apparatus, according to the method which I have called the *second* in my paper in the *Annals*, vol. xli. p. 226, with the application of a gilt condenser could, however, not demonstrate the slightest trace of electricity between gold and platinum (*Ann. de Chim. et Phys.*, xlv. p. 292, or *Traité de l'Electr.*, t. ii. p. 137), and attributes this to the want of the chemical action of the air upon both metals; while in fact, experiments with the multiplier, by the application of a fluid which would attack these metals, show that their combination is capable of exciting electricity. To this we may answer, that a multiplier only moderately sensitive is without comparison a more sensitive instrument for the demonstration of the weakest traces of electricity than the most sensitive condenser; therefore, by the undeniable only very weak electromotive oppositions of the gold and platinum, for which their chemical state itself speaks, it is very evident that the one instrument still indicates an action where the other would appear to show that none whatever takes place. Besides, the experiment of Becquerel completely loses all weight, for this reason; that he found in the same experimental series, that graphite and some other bodies, on which no chemical influence of the air can be proved, are capable of a development of electricity with gold, plainly recognisable with the condenser, even after they had previously been washed in distilled water; and I myself have already formerly men-

tioned the development of electricity between gold and silver, which may be demonstrated in a weak degree even without a condenser. If, indeed, De la Rive even in all these cases *supposes* (supponit) a chemical action, I have only to observe, that it must also be allowed me to suppose in all cases an electrical action, where it cannot be demonstrated, where further it agrees better with the contact theory.

6. Biot long ago convinced himself that piles constructed of an equal number of pairs of plates, with fluids of very different chemical action, as water, solution of common salt, of sal ammonia, of chloride of potassium, of sulphate of iron, indicated at the electrometer the same intensity of the poles. If other fluids, such as solution of soda, showed exceptions (which become explicable according to the theory of contact by a change of the metallic surface), these are certainly not of the kind that can speak in favour of the chemical theory. De la Rive himself performed similar experiments (*Recherch.*, p. 142) and found that piles constructed of an equal number of plates with river water, solution of Glauber's salts and greatly diluted nitric acid, indicated an equal power of electricity at the insulated pole, where (as was also the case in Biot's experiments) the other pole stood in connexion with the ground; if on the other hand both poles are insulated, a difference occurs, and the diluted nitric acid gives the weakest, often quite imperceptible electrical signs.

In order now to explain the equality of power, (for the first appearance is evidently opposed to the chemical theory) by construction of the pile with different fluids (in the case of non-insulation) De la Rive supposes that there is, it is true, more electricity developed by the liquids which attack more strongly, but that always one part of the developed opposite electricities reunites even through the members of the pile. But since fluids of stronger chemical action possess in general also a better conducting power, they would allow of a quicker reunion, and with this would be explained how the free portion of electricity is not stronger with them than with fluids which act less powerfully. Disregarding however other objections which might easily be raised against this view, it were yet very curious, if this compensation in the various fluids should amount exactly to the equality of the actions, and the more so, since the conducting power of the fluid members of the pile also depends in some degree on their dimensions; consequently the compensation could only be exact with one single thickness of the fluid layers. With the theory of contact the equality of the intensity of non-insulated piles in the construction with various fluids (in so far as they do not charge

their metallic surfaces) proceeds as a natural consequence without the aid of any hypotheses. The various action of the insulated pile is still enigmatic, but might possibly depend on this, that with a bad conducting fluid the amount of conduction of the pile becomes greater, since then the electricity probably penetrates even between fluid and metallic surface, which might be less the case if the conducting power of the fluid approached more to that of the metal; a subject moreover on which special experiments are still desirable.

[To be continued.]

XXVIII. *A New and General Solution of Cubic Equations.*

By JOHN T. GRAVES, of the Inner Temple, Esq., M.A.*

IN the ordinary books of algebra, (so far, at least, as my limited acquaintance with them extends,) where cubic equations are discussed, the cases of *real* coefficients only are considered, and different methods of solution are given in order to effect the separation between the constituents† of the roots in different cases. I have obtained a symmetrical solution of the equation

$$x^3 + (x + \sqrt{-1}\lambda)x + \mu + \sqrt{-1}\nu = 0 \quad (1.)$$

which presents the constituents of x in an explicit form in all cases. This is all that is wanted, for the solution of a cubic equation of the general form.

$$(\alpha_1 + \sqrt{-1}\beta_1)y^3 + (\alpha_2 + \sqrt{-1}\beta_2)y^2 + (\alpha_3 + \sqrt{-1}\beta_3)y + \alpha_4 + \sqrt{-1}\beta_4 = 0 \quad (2.)$$

may easily be made to depend on the solution of a cubic equation of the form (1.); and, from the nature of the relation between the transformed equation (1.) and the original equation (2.), the constituents (x, λ, μ, ν) of the transformed coefficients ($x + \sqrt{-1}\lambda, \mu + \sqrt{-1}\nu$) are easily determinable, supposing the constituents of the original coefficients to be explicitly given: and if the constituents of x be determinable, those of y can easily be determined.

The limits of this Magazine do not permit an exposition of my *process*, which I intend hereafter to communicate at length through some more appropriate medium. It consists in an analysis of the following formula for x .

$$x = -\sqrt{-1} \frac{2}{\sqrt{3}} p^{\frac{1}{2}} \cos \left\{ \frac{1}{3} \cos^{-1} \left(\sqrt{-1} \frac{3\sqrt{3}}{2} \frac{q}{p p^{\frac{1}{2}}} \right) \right\} \quad (3.)$$

where $p = x + \sqrt{-1}\lambda$ and $q = \mu + \sqrt{-1}\nu$, and where the

* Communicated by the Author.

† I call α and β the "constituents" of the expression $\alpha + \sqrt{-1}\beta$.

ambiguous $p^{\frac{1}{3}}$ may have either of its two values, provided it retain at any one time the same meaning in both places of its occurrence in formula (3.).

My *result* is presented in the following formulæ, in which $\sqrt[3]{}$ and $\sqrt[3]{}$ denote real roots not negative: \cos_0^{-1} denotes the smallest \cos^{-1} not negative: and i denotes any term of the arithmetical series $-2, -1, 0, 1, 2$, reckoned from 0 indefinitely backward and forward.

$$\text{Let } \left. \begin{aligned} Q_1 &= \frac{1}{\sqrt[3]{2}} \sqrt[3]{\sqrt{x^2 + \lambda^2} + x} \\ Q_2 &= \frac{1}{\sqrt[3]{2}} \sqrt[3]{\sqrt{x^2 + \lambda^2} - x} \end{aligned} \right\} \quad (4.)$$

$$\left. \begin{aligned} x_1 &= 2^{\frac{2}{3}} (x^3 - 3x\lambda^2) \\ \lambda_1 &= 2^{\frac{2}{3}} (3x^2\lambda - \lambda^3) \end{aligned} \right\} \quad (5.) \quad \left. \begin{aligned} \mu_1 &= 3^{\frac{2}{3}} (\mu^2 - \nu^2) \\ \nu_1 &= 3^{\frac{2}{3}} 2\mu\nu \end{aligned} \right\} \quad (6.)$$

$$\left. \begin{aligned} Q_3 &= \frac{1}{\sqrt[3]{2} \sqrt[3]{x_1^2 + \lambda_1^2}} \sqrt[3]{\sqrt{x_1^2 + \lambda_1^2} + \sqrt{\mu_1^2 + \nu_1^2} + \sqrt{(x_1 + \mu_1)^2 + (\lambda_1 + \nu_1)^2}} \\ Q_4 &= \frac{1}{\sqrt[3]{2} \sqrt[3]{x_1^2 + \lambda_1^2}} \sqrt[3]{-\sqrt{x_1^2 + \lambda_1^2} + \sqrt{\mu_1^2 + \nu_1^2} + \sqrt{(x_1 + \mu_1)^2 + (\lambda_1 + \nu_1)^2}} \\ Q_5 &= \frac{1}{\sqrt[3]{2} \sqrt[3]{x_1^2 + \lambda_1^2}} \sqrt[3]{\sqrt{x_1^2 + \lambda_1^2} + \sqrt{\mu_1^2 + \nu_1^2} - \sqrt{(x_1 + \mu_1)^2 + (\lambda_1 + \nu_1)^2}} \end{aligned} \right\} \quad (7.)$$

$$\left. \begin{aligned} R_1 &= \frac{1}{\sqrt[3]{3}} \left(\sqrt[3]{Q_3 + Q_4} + \sqrt[3]{Q_3 - Q_4} \right) \cos \left\{ \frac{1}{3} (2i\pi + \cos_0^{-1} Q_5) \right\} \\ R_2 &= \frac{1}{\sqrt[3]{3}} \left(\sqrt[3]{Q_3 + Q_4} - \sqrt[3]{Q_3 - Q_4} \right) \sin \left\{ \frac{1}{3} (2i\pi + \cos_0^{-1} Q_5) \right\} \end{aligned} \right\} \quad (8.)$$

$$\left. \begin{aligned} s_1 &= 2 Q_1^{\frac{2}{3}} (x\mu + \lambda\nu) + \lambda(x\nu - \lambda\mu) \\ s_2 &= 2 Q_2^{\frac{2}{3}} (x\mu + \lambda\nu) - \lambda(x\nu - \lambda\mu) \\ s_3 &= 2 Q_1^{\frac{2}{3}} (x\nu - \lambda\mu) - \lambda(x\mu + \lambda\nu) \\ s_4 &= 2 Q_2^{\frac{2}{3}} (x\nu - \lambda\mu) + \lambda(x\mu + \lambda\nu) \end{aligned} \right\} \quad (9.)$$

$$x = v_1 + \sqrt{-1} v_2 \quad (10.)$$

$$\text{Then } \left. \begin{aligned} v_1 &= \frac{s_1}{\sqrt[3]{s_1^3}} Q_1 R_2 + \frac{s_2}{\sqrt[3]{s_2^3}} Q_2 R_1 \\ v_2 &= \frac{s_3}{\sqrt[3]{s_3^3}} Q_1 R_1 + \frac{s_4}{\sqrt[3]{s_4^3}} Q_2 R_2 \end{aligned} \right\} \quad (11.)$$

From the preceding formulæ it would be easy, did the size of the page permit, to write out at full length a solution for x in immediate terms of x, λ, μ, ν . The relations *inter se* of the mediate functions employed are very remarkable. It will be seen that s_1, s_2, s_3 , and s_4 are wanted merely as *sign-indicators*. The critical cases where a sign-indicator becomes $= 0$, are im-

portant. It is also interesting to consider the conditions that are necessary in order that a root should be wholly real or wholly imaginary; and to observe the curious manner in which, when λ and ν are both $= 0$, the solution here given identifies itself respectively, according to certain relations between x and μ , with the ordinary algebraical or trigonometrical solution.

XXIX. *Note on the Constitution of Salts.* By Professor T. GRAHAM*.

THE author may perhaps be excused in drawing the attention of chemists to a distinction in saline combinations, which is at present too often overlooked, and confusion thereby occasioned. The orders of monobasic, bibasic, and tribasic salts, of which the phosphates proved types, have lately been greatly enlarged by the discoveries of Liebig and Dumas respecting vegetable acids, and the distinctive characters of these orders are well understood. The best proof of an acid's being bibasic or tribasic, is its combining at once with two bases which are isomorphous, or belong to the same natural family, as phosphoric acid does with soda and ammonia in microcosmic salt, and tartaric acid with potash and soda in Rochelle salt. Water and magnesia, water and barytes, water and oxide of lead, are also constantly associated as bases in bibasic and tribasic salts, but never in true double salts, or combinations of two or more salts with each other, with which salts of the preceding orders are apt to be confounded.

But it is too generally supposed, that a metallic oxide cannot exist in a saline combination, except in the capacity of base, although in most of those bodies which are at present termed subsalts, the whole or a portion of the metallic oxide is certainly not basic, but is attached to a really neutral salt in a capacity similar to that of constitutional water, or water of crystallization. Oxide of copper, oxide of lead, barytes, and the other metallic oxides included in or related to the magnesian family, appear to rival water (which is a member of the same family), in the frequency with which they discharge this function in the constitution of saline compounds, particularly of those belonging to the organic kingdom. Thus the neutral organic principle orcinic combines with five atoms of oxide of lead, according to Dumas, which replace five atoms of water which orcinic otherwise possesses. But it should be brought prominently into view, that neither the water nor the oxide of

* Read before the Chemical Section of the British Association, at the late meeting at Newcastle, Aug. 22, 1838; and now communicated by the Author.

lead is basic in these compounds, but superadded to the oricine like constitutional water; a distinction which is well expressed in their formulæ, by placing the symbols for water and oxide of lead after and not before that of oricine, or in the proper place for water of crystallization in the formula of a salt.

Potash, soda, oxide of silver, and oxide of ammonium, on the other hand, are never found in this relation to a salt, or discharging any other function than that of base to an acid. Hence there are no such compounds as subsalts of these bases.

In Peligot's late admirable paper on the varieties of sugar, (*Annales de Chimie*, &c., t. 67. p. 113*), he has formed the compounds of that principle with barytes, lime, oxide of lead, and common salt, and determined their composition with great accuracy. Like preceding chemists he considers these compounds as salts, in which sugar is the acid and the metallic oxide the base, and continues to speak of them as saccharates, although with an evident reserve. But the conclusion is by no means necessary that sugar is an acid, and that the lime, oxide of lead, &c. are basic to it. On the contrary, sugar being a body neutral to test paper, is more likely to be a salt than an acid. That the metallic oxide attached to it discharges the function of the superadded water of crystallization of so many bodies, appears to me evident from the following circumstances.

1. It is separated from the sugar by the weakest acids, even by carbonic acid.

2. It replaces water in the sugar, which water can also be replaced in part by an equivalent proportion of chloride of sodium, or by the *hydrates* of barytes and lime. Now basic water is never replaced by a salt, although constitutional water frequently is.

3. But the circumstance which is decisive of the lime and oxide of lead not being basic in the sugar compounds is, that analogous compounds do not exist, containing potash or any of the strong alkaline bases of its class. No acid is known which forms a salt with lime or lead, that does not also form a salt with potash or soda; but these last, as has been stated, are never present in any other capacity than that of bases, and are thus disqualified from replacing the water or magnesian oxide in combination with sugar.

The test of the non-basic character of water or a metallic oxide in a compound, is the absence of a parallel combination containing an oxide of the potash class.

The fact that the combined water in sugar is strongly attached and cannot be removed by heat, is no proof that the

* A notice of M. Peligot's researches on this subject will be found in the present Number, p. 237.—EDIT.

water is basic; for many nitrates, hyposulphites, &c., are known, the constitutional or superadded water of which cannot be removed by the same agency without destroying the salts.

XXX. Notices respecting New Books.

A Brief Account of the Life, Writings, and Inventions of SIR SAMUEL MORLAND, Master of Mechanics to Charles the Second.

RARA MATHEMATICA; or, *A Collection of Treatises on the Mathematics and subjects connected with them, from ancient inedited manuscripts.* No. I.—Deightons, Cambridge; Parker, London.

THESE two works are anonymous; but they carry with them sufficient evidence of a close intimacy with the subjects to which they relate.

The little attention paid by Englishmen to the history of science in England is not a new subject of reproach. Almost every human pursuit has had its history investigated, its fragments published, and its cultivators biographed, except science: perhaps this class of researches requiring a combination of knowledge and tastes that rarely go together, may be the chief cause. There is so little seeming, so little real, fraternity between the taste for decyphering ancient manuscripts, and that for pure scientific investigation, that we hail this class of publications as a real boon to the reputation of England.

Of the first of these works, it will be unnecessary to say more than that it is a succinct history of the life of a man of great powers of mind, whose life was marked by the usual vicissitudes of the followers of a court in perilous times, and who was reduced at the end of his career to penury. Attached to this, is a short and interesting discussion of some points connected with the early history of arithmetic.

The *Rara Mathematica*, No. 1. contains:—

1. Sacro-Bosco de Arte Numerandi.
2. A Method used in England in the Fifteenth Century for taking the Altitude of a Steeple or inaccessible object.
3. A Treatise on the Numeration of Algorithm; from a MS. of the fourteenth century.
4. William Bourne on Optic Glasses; written about 1580.
5. Johannes Robyns de Cometis.

We think it unnecessary to say one word concerning the historical value of these tracts, as their importance will be at once admitted. We shall, however, on a future occasion give an analysis of them, and of such others as may appear in subsequent numbers; but we may here express our anticipation of their furnishing materials for a decision upon some important points connected with scientific history, which have been, as yet, but very vaguely discussed.

Finally, a work like this may safely repose upon its own intrinsic value, without any recommendation of ours; and we have therefore merely felt it necessary to call the attention of our readers to its ex-

istence. The chief objects to be kept in view are, the historical importance of the tracts selected for publication, and carefully decyphering the doubtful contractions that occur in MSS. of the period which this collection is intended to include. The present number being taken as a specimen, we are sure the execution is in good hands.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 152.]

May 31.—A paper was read, entitled, “Remarks on the Theory of the Dispersion of Light, as connected with Polarization.” By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

The present paper is a sequel to those already presented by the author to the Royal Society, in which he had instituted a comparison of the observations of the refractive indices for the standard rays of light in various media, with the results calculated from theoretical formulæ, deduced from the most improved views of the undulatory hypothesis; the cases discussed including the greatest range of data as yet furnished by experiment. The comparison exhibited an accordance sufficient to warrant the conclusion that the theory affords a very satisfactory approximation, at least, to the expression and explanation of the actual law of nature*. In order, however, to remove any possible discrepancy which may still exist, or hereafter be found to obtain, the author considers that further examination is requisite of the principles on which any extension or modification of the theory might be pursued; and such is the object of the investigation undertaken in the present paper.

The phenomena of interference, on which the undulatory theory was originally based by Dr. Young, obliged us to adopt some idea of an alternating motion, as well as a motion of translation, in our conception of light; and this, with all the accessions it has received, especially from the investigation of Fresnel, has, at the present day, been connected by the labours of M. Cauchy and others, with general dynamical principles, which regulate the propagation of vibratory motions through an elastic medium. From such dynamical principles there have been deduced certain differential equations of motion, the integration of which gives the well-known expression for a wave, involving the relation between the velocity and the wavelength which explains the dispersion. The direct and complete integration of these forms, effected by M. Cauchy †, and simplified by

* A notice of Prof. Powell's last paper on the subject was given in vol. xii. p. 367.; where also will be found references to abstracts of the three former.—EDIT.

† Prof. Powell's “Abstract of the Essential Principles of M. Cauchy's View of the Undulatory Theory,” appeared in Lond. and Edinb. Phil. Mag. vol. vi. p. 16. *et seq.*

Mr. Tovey* and M. Kelland†, involves certain conditions; namely, the evanescence of certain terms, the interpretation of which implies peculiar views of the constitution of the ether. Mr. Tovey shows that without these conditions, a certain form of the wave-function is a particular solution of the equations; and this form is precisely that expressing elliptically polarized light. If the absence of the condition in question be essential to the case of elliptically and circularly polarized light, it follows that all the preceding investigations, which depend on the fulfilment of those conditions, are applicable only to unpolarized and plane-polarized light, and consequently the general integration is limited in a most material part of its application; a defect which is only remedied by the supplementary investigation of Mr. Tovey, in which, for this case, a particular solution is assigned. It seemed, then, necessary to show explicitly that the non-fulfilment of the conditions, that is, the non-evanescence of the terms in question, is essential for elliptically polarized light, as their evanescence is for common light, and thus to exhibit distinctly the relation between the cases of elliptically polarized, of plane-polarized, and unpolarized light; and, again, to remove, if possible, the obscurity and discrepancy of opinion in which the physical interpretation of those conditions, with regard to the supposed constitution of the ethereal medium, appeared to be involved.

The author then enters upon the analytical investigation of the subject, and in conclusion remarks that when light is elliptically or circularly polarized, that is, when any one of the two component vibrations is retarded behind the other, then, in the differential equations of motion, the opposite terms do not destroy each other in the summation, which they can only do in general by supposing a great number taken into account; that is, the number of terms is limited, or the sphere of the influence of the force by which the vibrations are propagated is small. When light is plane-polarized, or unpolarized, that is, when there is no retardation, or the phases of the component vibrations are simultaneous, then the opposite sums destroy each other; that is, the number of terms involved is greater, or the sphere of the influence of the force greater. Since both kinds of light can be propagated indifferently through ordinary media, it follows that the sphere of influence of the force, or number of molecules taken into account, does not here depend on the arrangement of the molecules of ether in the medium, but on the retardation of one of the vibrations behind the other, or the absence of it, originally impressed on the ray in the respective cases.

A paper was also read, entitled, "An Experimental Inquiry into the influence of Nitrogen on the Growth of Plants." By Robert Rigg, Esq. Communicated by the Rev. J. B. Reade, M.A., F.R.S., &c.

The author, after briefly alluding to a former paper laid before the Royal Society, describing the chemical changes which occur during

* Mr. Tovey's investigations on this subject have appeared, exclusively, we believe, in this Journal: see Lond. and Edinb. Phil. Mag., vol. viii. p. 7., 270, 500.; vol. ix. p. 420.; xi. 524.; and xii. p. 10, 259.

† Prof. Kelland's development of his views will be found in vol. x. p. 336.

the germination of seeds, and some of the decompositions of vegetable matter, proceeds, in the present paper, to trace a connexion between the phenomena exhibited during the growth of plants, and the direct agency of nitrogen. The experiments by which the author supports his views are arranged in separate tables, so drawn out as to indicate not only the quantities of carbon, oxygen, hydrogen, nitrogen, and residual matter, in about 120 different vegetable substances, but also the quantity of nitrogen in each compound, when compared with 1000 parts by weight of carbon in the same substance. The most important of these tables are those which exhibit the chemical constitution of the germs, cotyledons and rootlets of seeds; the elements of the roots and trunks of trees, and the characters of the various parts of plants, especially of the leaves, at different periods of their growth. From this extensive series, which is stated to form but a small portion of the experiments made by the author in this department of chemical research, it appears that nitrogen and residual matter are invariably the most abundant in those parts of plants which perform the most important offices in vegetable physiology; and hence the author is disposed to infer, that nitrogen (being the element which more than any other is permanent in its character) when coupled with residual matter, is the moving agent, acting under the living principle of the plant, and moulding into shape the other elements. The method of ultimate analysis adopted by the author, enables him, as he conceives, to detect very minute errors, and therefore to speak with certainty as to the accuracy and value of every experiment*.

A paper was also read, entitled, "*Researches in Rotatory Motion.*" By A. Bell, Esq. Communicated by the Rev. W. Whewell, M.A., F.R.S., &c.

This paper, which is altogether analytical, contains several new theorems in rotatory motion, respecting the effect of the centrifugal force arising from a rotation about any axis, in producing rotation about another, inclined at any angle to the former; and also a new, and comparatively concise, demonstration of the equations of the motion of rotation of a solid body, its centre of gravity being fixed, and the body being acted on by any forces.

June 14.—A paper was read, entitled, "*Researches on Suppuration;*" by George Gulliver, Esq., Assistant Surgeon to the Royal Regiment of Horse Guards. Communicated by John Davy, M.D., F.R.S., Assistant Inspector of Army Hospitals.†

A paper was also in part read, entitled, "*Researches on the Tides,*" Ninth Series; by the Rev. W. Whewell, M.A., F.R.S., &c.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The Eighth Meeting of the British Association took place at Newcastle-upon-Tyne, in the week from the 20th to the 25th of

* An abstract of Mr. Rigg's paper on the germination of seeds will be found in Lond. and Edinb. Phil. Mag. vol. ix. p. 536: see also vol. xii. p. 31, 232.

† This paper will be found, entire, in the present number, p. 193.

August; the General Committee having met for the first time on Saturday, August 18th, on which occasion the chair was taken by Prof. Whewell, V.P., in the absence of the Earl of Burlington, President; and the Rev. J. Yates, the Secretary to the Council, read the report of the proceedings of that body for the past year. The first general meeting assembled in the Central Exchange on the Monday evening, August 20th, more than 3200 persons being present, when Prof. Whewell resigned the chair to the Duke of Northumberland, the new President, and Mr. Murchison, the General Secretary, read his report, giving, agreeably to the directions of the Council, "a general and comprehensive view of the past progress and future prospects" of the Association. The several Sections were presided over by the following men of science: Section A, Mathematical and Physical Science, Sir John F. W. Herschel, Bart.; B, Chemistry and Mineralogy, the Rev. W. Whewell; C, Geology and Geography, President for Geology C. Lyell, Esq., President for Geography Lord Prudhoe; D, Zoology and Botany, Sir W. Jardine, Bart.; E, Medical Science, Dr. Headlam; F, Statistics, Col. Sykes; G, Mechanical Science, Charles Babbage, Esq. The continued progress of the Association was evinced by the number of tickets issued for members, which, up to the Wednesday evening, was 2350, being upwards of 500 more than the number at the close of the meeting at Liverpool last year. A paper read before Section B, appears in our present number, p. 219.

GEOLOGICAL SOCIETY.

(Continued from vol. xii. p. 592.)

April 4.—A paper was read entitled, "A Description of Viscount Cole's specimen of *Plesiosaurus macrocephalus*, (Conybeare's)," by Richard Owen, Esq., F.G.S., Hunterian Professor in the College of Surgeons, London; an abstract of which will be found in the "Proceedings" of the Society, No. 57, and also in the Annals of Natural History for September.

April 25.—A paper was first read, entitled, "Notes on a small patch of Silurian Rocks to the west of Abergele, on the north coast of Denbighshire;" by J. E. Bowman, Esq., and communicated by R. I. Murchison, Esq., V.P.G.S.

The author's attention was first directed to these strata by Mr. John Price, of New College, Bristol. They occur immediately south of the narrow belt of carboniferous limestone, which skirts the coast from the Great Ormes Head, eastward, to the Point of Air and the Estuary of the Dee. The belt of limestone is here not above a mile broad, and the strata dip N. or N.E. At the base of the limestone precipices at Craig y Forwyn, is a seam of impure coal about a foot thick, and a thinner layer of bituminous shale with carbonized impressions of *Lepidodendra?* and a leaf-like *Poacites*. The beds constituting the following section are successively displayed between Llandulas and Garthewin, a distance of nearly six miles:—

1. Immediately under the limestone is a conglomerate, the basis consisting of "light loam," and the rounded pebbles of

greenish, slightly micaceous sandstone, containing a few bivalves and joints of encrinites. This stratum the author has also seen between Llandeilo Bay and Colwyn on the Holyhead road, but the pebbles are there sometimes a foot in diameter.

2. Thin bed of the same sandstone.
3. A thick deposit of red marl, containing numerous angular and water-worn pebbles, interspersed thickly with shells apparently belonging to the Ludlow rocks. This marl forms a considerable part of Ffernant Dingle; but it alternates with a compact marl, and is sometimes speckled green or yellow. The beds dip at a high angle to the north. Similar pebbles are found on the top of the limestone precipices, and beyond them on the beach.
4. Compact, hard, arenaceous conglomerate, composed of pebbles more or less rounded, of liver-coloured and green micaceous shelly sandstone, also of pebbles of quartz, and the reddish subjacent limestone.
5. Thin beds of compact reddish limestone containing few organic remains. It passes occasionally into a calcareous sandstone.
6. Near the lower end of the dingle, the limestone rests on a bed of very fine blue clay.
7. Blue clay-slate, finely grained, slightly micaceous, and containing occasionally a layer of small shells. It sometimes presents obscure indications of vertical cleavage. This rock constitutes the whole of the southern portion of the dingle, and in one place is traversed by a fault.

The rill in Ffernant Dingle flows into Melin y Person brook. The red marly conglomerate is there succeeded, on the south, by alluvium containing slate pebbles. Above the village of Bettws Abergele the slate rocks occur, but are greatly contorted; and on the height a little further south, and to the east of the road, is a hard finely grained rock inclosing joints of small encrinites. Still further south this rock alternates with beds of breccia, containing encrinital and other organic remains, the imbedded angular fragments consisting of glossy clay-slate. A little south of this quarry, towards Garthewin, the non-fissile blue slate again occurs, and the author found in it abundance of small fragments of encrinites, with univalves and bivalves. These fossils occurred apparently in layers, but were much decomposed. Similar remains were noticed, by Mr. Bowman, in the debris of the lead mines at the Bronhaylog, to the north-east of Garthewin.

The paper was accompanied by a list of fossils prepared by Mr. James de Carle Sowerby, including the following species which have been found by Mr. Murchison in the Ludlow Rocks elsewhere: *Leptanatala*, *Terebratula nucula*, *T. pulchra*, *T. navicula*, *Conularia quadrisulcata*, *Atrypa affinis*, *Orthis orbicularis*, *Cypricardia cymbaformis*, var., *Nucula ovalis*, *Euomphalus funatus*, *Orthoceras striatum*, *Avicula retroflexa*, and *Pleurotoma corallii*.

A notice "On the Occurrence of Wealden strata at Linkfield, near Elgin; on the Remains of Fishes in the Old Red Sandstone of that neighbourhood; and on raised beaches along the adjacent coast;" by J. Malcolmson, Esq., F.G.S., was then read.

The country around Elgin is composed of sandstones, conglomerates, and concretionary limestones, belonging to the old red sandstone; but at Linkfield, one mile south of Elgin, that formation is overlaid, unconformably, by a series of beds, which Mr. Malcolmson has ascertained, by their organic remains, to represent the Wealden strata of England, though they have been usually considered to be lias.

The following section gives the principal beds in descending order, the average thickness of the whole series being from 20 to 30 feet:

1. Blue clay, containing thin bands of limestone, the lower being shelly.
2. Thin bands of limestone and clay.
3. Blackish shale, not bituminous, 1 to 2 feet.
4. Compact grey limestone, without shells, in layers separated by clay, 4 feet.
5. Laminated green clay, with a network of fibrous carbonate of lime.
6. Red, sandy, calcareous marl, abounding with rolled pebbles of granite, gneiss, &c., also angular fragments of the fine-grained yellow and grey sandstone forming the hills to the west, but the geological position of which is not yet ascertained.

Cornstone of the old red sandstone in unconformable position.

The fossils are principally found in the lower bands of the top bed. They are rarely well preserved, and cannot be separated from the rock. The species are few in number, but abundant in individuals; and one species of *Cyclas* is undistinguishable from the *C. media* of Sussex, found also by Prof. Sedgwick and Mr. Murchison in the Isle of Skye: there is likewise an *Avicula*, which agrees with one found in the Purbeck strata at Swanwich. Mr. Malcolmson procured also fragments of an *Astarte* and a *Venus*, and a microscopic univalve. The clay below this shelly limestone is full of the valves of a new, round species of *Cypris*. The author also obtained teeth and scales of fishes; and the Rev. G. Gordon has found a Saurian bone.

Fossils of the same description have been recently discovered by that gentleman at Lhanbryde, three miles to the east of Linkfield; and in a micaceous white sandstone, he has procured a large *Pinna*, which Mr. James Sowerby has identified with a species found in the Portland sand of England. In April, 1832, Mr. Gordon communicated to the Society a notice of the discovery in a dark clay*, penetrated while draining the Lake of Spynie, of the *Turritella muricata* of the Coral Rag. Mr. Malcolmson, therefore, hopes that many members of the series above the old red sandstone, not yet known to exist south of the Murray Frith, will be discovered by the practical geologists resident in that district.

Mr. Martin, of the Anderson Institution, has recently discovered in a bed of calciferous conglomerate, near Elgin, and supposed by Mr. Gordon to represent the old red sandstone of Clashennie in

* Proceedings, vol. i. p. 394; or Lond. and Edinb. Phil. Mag. vol. i. p. 227.

Perthshire, scales, teeth, and bones of fishes; and, by comparing these remains with a magnificent specimen of a fish from Clasbennie, in Mr. Murchison's possession, Mr. Malcolmson has ascertained this supposition to be correct. A doubt, therefore, which formerly existed respecting the age of the conglomerate, is now removed.

The paper concluded with an account of eleven ancient beaches on the coast, rising above each other, and from one of which, 15 feet above high-water mark, and cut through in draining Loch Spynie, Mr. Malcolmson procured twelve species of existing marine testacea.

A paper, "On the Origin of the Limestones of Devonshire," by Robert Alfred Cloyne Austen, Esq., F.G.S., was afterwards read.

The object of the paper is not to account for the origin of calcareous matter, or the means by which marine animals derive it from the surrounding medium, but to show how far the limestones of South Devonshire may have been produced by polypti.

These limestones are stated by the author to occur, in nearly every instance, in the immediate vicinity of volcanic disturbances, and to be partly included in the slates and sandstones, and partly to rest upon them. To the former belong the broad band extending from Staple Hill to Dean Prior, the minor bands in the neighbourhood of Hempstone and Totness, and all those which occur beyond the Dart; also the limestones of Newton and Torbay. They are said to be less pure and more slaty than the overlying limestones, and to be frequently separated by seams of shale. Transverse sections of these bands show, that the strata in some cases become thinner as they descend, and that the partings of shale increase, as near Staverton in the valley of the Dart, and at Staple Hill; but that in other instances, as between Newton and Totness, the strata instead of fining off end abruptly upon the slate, and are covered in the direction of the dip by similar slates. The strata are always inclined, but they invariably form a table-land at the surface. This inclined position the author conceives is not due to dislocation, but to the beds having been deposited at the angle which they now present; and he illustrated his opinion, by a section between three and four miles in length, through the parishes of Pegwell, Denbury, and Abbots Kerswell, a remarkably level country. The bands of limestone dip 40° , but are nowhere more than 150 feet thick, and they all contain the same description of organic remains. If the bands were deposited horizontally, and the most recent nearly at a level with the surface of the ancient ocean, then the lower beds, the author says, would have been placed at a depth of nearly three miles, although the organic remains prove that all the beds were formed under precisely similar conditions.

In the structure of the Devonshire limestones, however, Mr. Austen considers that he has discovered evidences of an origin similar to that of modern coral reefs, and which will explain their inclined position. At Ogwell Park the limestone forms a horizontal capping to the inclined strata; and at Bradley rests conformably against a ridge of slate the basset edge of each bed rising to the level of the crest of the ridge.

This structure, Mr. Austen states, agrees with that of the coral reefs in the Southern Ocean, where the polypi raise their habitations on the flat summits or sides of submarine hills, to a level with the surface of the water. The stratified arrangement of the calcareous masses he considers may be explained by the occasional deposition of sedimentary matter, which might interrupt, for a time, the labours of the polypus; and thus a series of beds would be produced varying in thickness according to the recurrence, at shorter or longer intervals, of interfering agents, each bed rising successively to the surface level of the water. If the deposition of sedimentary matter were great, then the polypi would be destroyed, and the reef would become encased in a mechanical accumulation. In further proof of the limestone of Devonshire having been coral reefs, Mr. Austen adduced the great abundance of zoophytes found on the surface of the lower strata, imbedded in the layers of sand which separate the beds; and, he added, that their absence in other parts, especially in the interior of the bands, is no objection to his view of the origin of the limestone, because, in recent reefs, all traces of organic structure are frequently obliterated.

May 9.—A communication by Dr. Black, F.G.S., was first read, "On a fossil stem of a Tree recently discovered near Bolton-le-Moor."

The rock in which this fossil was found, occurs in the middle of the coal-measures, about 50 yards beneath a six-feet bed of coal, and it rests upon another bed four feet thick. It consists of three strata of argillaceous sandstone dipping from 15° to 18° to the south-west, and amounting in all to about 40 feet in thickness. The upper portion of the fossil stem was discovered about thirty feet beneath the surface of the rock, and the lower end extended to within 5 or 10 feet of the subjacent bed of coal. It was inclined 18° to north-east, or in an opposite direction to the sandstone strata; and, when first laid open, it appears to have been about 30 feet in length, but at the time it was examined by Dr. Black only 12 feet remained *in situ*. The upper end of this portion was 15 inches in diameter, and the lower 9 inches. The whole of the exterior of the stem was singularly striated, and irregularly furrowed, as if by compression; and it was coated with a layer of coal, which evidently occupied the place of the bark. The interior of the stem is stated to be composed of a dark, hard, argillo-ferruginous sandstone, having a specific gravity of 2.9. A *Sternbergia*, about an inch in diameter, extended along the whole length of the stem, and in some parts appeared to be half imbedded in a groove in it. This connexion of the two plants was Dr. Black's principal object in making the communication to the Society, not having previously observed a similar occurrence, nor having heard that it had been noticed elsewhere by other collectors. He is of opinion that the *Sternbergia* was not accidentally allocated with the larger stem, but that it was, while living, a parasite, and in this respect resembled the mighty creepers of the existing tropical regions.

A paper was next read, "On the Distribution of Organic Remains in part of the Oolitic Series on the coast of Yorkshire;" by Mr. Williamson, Curator of the Natural History Society of Manchester.

In former communications* Mr. Williamson explained the vertical range of organic remains in the Lias and inferior and great oolites, and in this he showed their distribution in the upper sandstone and shale, the cornbrash, the Kelloway Rock, and the Oxford Clay.

The upper sandstones and shales vary considerably in their characters, but they consist of three principal divisions, the highest and lowest being composed of sandstones sometimes ferruginous, and the middle one of clays and shales. The principal localities for the fossils are on the north side of Scalby Beck, near Scarborough, and Burniston Bay. The most characteristic plants are *Pecopteris Murrayana*, *Cyclopteris digitata*, and *Otopteris obtusa*; but remains of Cycadææ and Equisetæ also occur. The list of plants is much smaller than that generally given, in consequence of Mr. Williamson having removed, to the great oolite, a bed generally considered as belonging to the upper sandstones.

Cornbrash.—This formation seldom exceeds five feet in thickness, and in Cayton Bay consists of the following strata in descending order:

| | |
|------------------------------------|---------------------|
| Fissile oolite | 6 inches. |
| Softer rock, sometimes ironshot. . | 2 feet. |
| Hard ironshot rock. | 2 feet. |
| Blue clay, from | 3 inches to 4 feet. |

The fossils contained in the fissile upper bed, are chiefly *Terebratula ovoidea*, *T. obsoleta*, *Ostrea edulina*, and *O. Marshii*. The greater portion of the organic remains are found towards the middle of the deposit, the following being the most abundant: *Ammonites Herveyi*, *Ostrea Marshii*, *Plagiostoma rigidulum*, *P. interstinctum*, *Trigonia clavata*, *T. costata*, *Cardium citrinoides*, *Unio peregrinus*, *Amphidesma decurtatum*, *A. securiforme*, *Mya literata*, and *Clypeus orbicularis*. Twenty other species also occur, but less numerous. In the bed of clay, remains of a small *Astacus* (?) are obtained, also a shell resembling an *Unio*, and an undescribed Belemnite. Thirteen of the species found in the cornbrash are stated to exist also in the great oolite of Yorkshire, and nine in the coralline or Oxford oolite.

Kelloway Rock.—This deposit consists of soft sandstones, sometimes calcareous, but towards the top it is occasionally very ferruginous; and it varies in thickness from 35 to 70 feet. The fossils are numerous and highly characteristic, particularly the Ammonites. The ferruginous bed is full of organic remains, consisting chiefly of *Belemnites abbreviatus*, *B. tornatilis*, *Ammonites Calloviensis*, *A. sublævis*, *A. Königi*, *A. Sutherlandæ*, *Ostrea Marshii*, *Gryphea dilatata*, β . The most abundant species in the sandstones are, *Ammonites flexicostatus*, *A. sublævis*, *A. gemmatus*, *A. Calloviensis*, *A. perarmatus*, *A. ichthyodorus* (W.), *A. gamma* (W.), *A. rotifer* (W.), *A. obliquus* (W.), *Belemnites abbreviatus*, *B. tornatilis*, *Turbo sulcostomus*, *Terebratula ornithocephala*, *T. socialis*, *Gryphea dilatata*, *Ostrea Marshii*, *Avicula Braamburiensis*, *A. expansa*, *Lucina lyrata*, *Amphidesma recurvum*: 18 other species of testacea occur, though less abun-

* Proceedings, vol. ii., pp. 82, 429; or Lond. and Edinb. Phil. Mag. vol. v. p. 222, vol. x. p. 137.; Geol. Trans. Sec. Ser. vol. v. Part I., p. 223, et seq.

dantly. Five species are stated to be common to the Kelloway rock and the cornbrash, and ten to the Kelloway rock and the coralline oolite. Remains of fishes and of Ichthyosauri and Plesiosauri also occur in the deposit.

Oxford Clay.—This great argillaceous formation is about 130 feet thick, and consists chiefly of fissile shales, but towards the upper part it becomes sandy. Fossils are comparatively rare in it, and are confined to the lower part, the only shell discovered in the upper by Mr. Williamson being *Pinna lanceolata*. The characteristic fossils are *Ammonites Vernoni*, *A. cristatus*, *A. athleta*, *A. occulatus*, *Belemnites gracilis*, *Nucula nuda*, *N. elliptica*, *Pinna mitis*, *Astarte lurida*, *A. carinata*, *Avicula expansa*. The bed resting on the Kelloway rock is characterised by *Belemnites abbreviatus* and *Gryphæa bullata*.

In future communications, the author purposes to illustrate the distribution of organic remains in the higher oolitic strata of the Yorkshire coast.

A paper was afterwards read, "On the State in which Animal Matter is usually found in Fossils;" by Mr. Alfred Smee, Student of King's College, London, and communicated by Prof. Royle, M.D., F.G.S.

The author first describes briefly, the composition of those parts of recent animals capable of being preserved in a fossil state; and then proceeds to detail his investigations into the composition of fossil organic remains.

For the sake of arrangement, he divides fossils into two great classes, one in which animal matter is present in various states, the other in which it has been removed. The first class he further subdivides into three cases: 1. comprehending those fossils in which animal matter retains its original condition; 2. those in which it has been partially changed; 3. those in which only the carbon of the animal matter remains.

1. The following examples were given of the first case.

Small portions of the tooth of a horse, of an ox, and a stag, from the chalk rubble at Brighton, were submitted to the action of diluted muriatic acid; and after the earthy portions had been removed the animal matter retained the shape of the bone, was white, and of the consistence of cartilage. Fragments of a tooth of a mammoth from Norfolk, and of a rib of a mastodon from Big-bone-lick in Ohio, when similarly treated, gave the same results. A thin slice of the rib exhibited under the microscope the structure of recent bone. Fragments of a stag's rib and horn, of an ox's head, and the tusk of a boar found near the Bank of England, associated with Roman implements, retained their animal matter unaltered. Small portions of a *Terebratula* and of two species of *Productæ*, from the Silurian rocks of Malvern, were placed in very diluted muriatic acid, and when the earthy portions had been removed, small flocculi of animal matter, resembling the recent membrane of a shell, floated in the solution. A minute fragment of *Asaphus caudatus* yielded little shreds of animal matter. The experiments on the shells were repeated several times with the same results. Under the microscope these fossils exhibited also the structure of recent shells.

2. The second case, in which animal matter has been partially changed, was illustrated by the following experiments. Portions of a stag's jaw from the Brighton chalk rubble, of a fish-bone, and a shark's tooth from the London clay, when dissolved in diluted muriatic acid, gave only a brown powder; and the animal matter of a fragment of the humerus of a mastodon from Big-bone-lick exhibited but little flexibility, and was easily torn, particularly in the longitudinal direction. It was found impossible to make sections of the jaw-bone of the stag or the humerus of the mastodon for microscopic observation. Part of a human parietal bone found upon the site of the cathedral of Old Sarum, and human bones obtained from the church-yard of St. Christopher le Stocks, on part of which the Bank of England stands, were ascertained to have had their animal matter reduced to the same state as that of the stag's jaw. A fossil oyster from the Isle of Wight, when placed under the microscope, showed black spots over its surface, and the structure of the shell was apparently destroyed. A fragment of a Pecten from the lias also exhibited opaque spots. Part of an ammonite when dissolved left a substance resembling Sepia.

3. The third case, where only the carbon of the animal matter remains, was explained by two series of experiments, one of which proved it to be associated with bitumen, and the other that it existed by itself. The scales of *Dapedium politum* and other fishes from Lyme Regis, when acted upon by acid, left carbon undissolved; and when heated under a test-tube gave a considerable quantity of bitumen*.

Portions of the bones of the Ichthyosaurus and Plesiosaurus from the lias, yielded a black residuum, which deflagrated with red hot nitre, and the resulting mass gave a precipitate with chloride of calcium. To prove that the carbon was a portion of the bone and not an adventitious ingredient, a section was made, and the greatest quantity of carbon was found in the thickest part; and an analysis showed that the proportion of carbon was about the same as in the animal matter of a similar mass of recent bone. A still further proof was adduced, in no gelatine having been detected after 36 hours boiling of a fragment of the fossil. A section of recent bone displayed, when carbonized by heat and charged with crystals of alum or a composition of whitening, a similar appearance in the arrangement of the carbon as in the fossil bone. No bitumen was given off, when fragments of these bones were acted upon by heat under a test-tube.*

With respect to the second great class in which the animal matter has been removed, the following cases were mentioned:—Portion of

* [We presume it is here intended to be implied, though it is not so stated, that the bitumen associated with the carbon in these fossil fish-scales is itself a part of the altered animal matter, in a state of imperfect carbonization. Such an inference would appear to be confirmed by the negative result obtained with the bones of the Ichthyosaurus and Plesiosaurus subsequently stated. See the observations of Mr. Murchison and Mr. Faraday on the origin of the bitumen in the bituminous schist of Seefeld, in Phil. Mag. and Annals, N.S., vol. vi. p. 39.—EDIT.]

the external and internal parts of a mammoth tusk from Siberia, did not blacken by heat, and dissolved completely in muriatic acid. The internal part of a tusk from Ohio gave the same results, but the external part was found to contain a considerable proportion of animal matter. In bones from the crag, the animal matter had been abstracted. Human bones which had been long buried were found to be in the same state.

The paper concluded with the following remarks. As the different states, in which animal matter is found in fossils, pass insensibly into each other, and as many of the changes occur in church-yard and other bones, it follows, that no extraordinary circumstances are requisite to produce these alterations; but that they may be effected by the ordinary processes of putrefaction. Even the carbonization of animal matter may be accomplished by similar processes without the aid of heat, as bones become black by being macerated too long. It is also to be observed, that the parts of animals preserved in the fossil state, are those which longest resist putrefaction. It having been likewise shown that the degree of change does not depend upon the age of the bed in which the fossil occurs, it is a curious subject of inquiry for the geologist to ascertain how far the conditions necessary to putrefaction, air, a certain temperature, and moisture, were present in those strata, in which the change has been great; how far they were absent in those, in which the change has been small.

XXXII. *Intelligence and Miscellaneous Articles.*

THE SWISS ASSOCIATION FOR THE ADVANCEMENT OF NATURAL SCIENCE.

This Association will hold its Annual Meeting on the 12th, 13th, and 14th of this month at Bâle, that is to say, a few days after that of the French Geological Society at Purrentray in the Swiss Jura (which is about 14 leagues from Bâle), and two days previous to the opening of the German Association at Fribourg in Baden (about 12 leagues from Bâle).

ERRORS IN THE NOMENCLATURE OF CERTAIN STARS IN GROOMBRIDGE'S CATALOGUE.

The following notice has been inserted in the monthly notices of the Astronomical Society for March, at the request of the Astronomer Royal: we transfer it to our pages for the purpose of giving it further publicity.

Immediately after sending out a number of copies of Groombridge's Catalogue, I discovered that some errors had been committed in the nomenclature of the stars, with reference, chiefly, to their accordance with the corrections made by Mr. Baily in Flamsteed's British Catalogue. These errors arose from the omission of a comparison which was supposed to be fully included in another.

I have since made a collation; and I think it probable that the following list contains the whole of these errors.

Groombridge, No. 462 and 463 constitute 59 Andromedæ.

| | | | |
|---------------|--------|-----|---------------------------|
| 1172 | | is | 7 Lyncis. |
| 1297 and 1298 | | are | 20 Lyncis. |
| 1478 | is not | | 7 Ursæ Majoris. |
| 1984 | is not | | 22 Canum Venaticorum. |
| 2185 | is | | 8 Ursæ Minoris. |
| 2780 | is not | | 19 Lyræ. |
| 3196 | is not | | 44 Cygni. |
| 3412 | is not | | 68 Cygni or Bradley 2775. |
| 3427 | is | | 68 Cygni, Bradley 2775. |
| 4240 | is | | Bradley 3216. |
| 4241 | is | | Bradley 3217. |

March 9, 1838.

G. B. AIRY.

ON THE CHEMICAL REACTIONS OF WATER. BY M. KUHLMAN.

The influence exerted by the action of water in some chemical reactions, has already been the subject of several important observations. Proust has shown that nitric acid of sp. gr. 1.48 does not attack tin, and that by the addition of a little water its action is extremely energetic. M. Pelouze has more recently stated some other facts: 1st. That acetic acid, of sp. gr. 1.063, does not decompose carbonate of barytes; 2ndly. That the carbonates of potash, soda, lead, zinc, strontia, and magnesia, are decomposed by crystallizable acetic acid; but that the energy of the action is greater when water is added, and that there is no action upon these carbonates when the acetic acid is mixed with absolute alcohol; lastly, That anhydrous alcohol, sulphuric æther, and acetic æther, completely mask the properties of the most powerful acids; their solutions do not redden litmus, and do not attack a great number of carbonates.

The rational explanation of so strange a fact (the non-action of acetic acid mixed with alcohol upon carbonate of potash) is not readily found. The intervention of insolubility, as likely to oppose the formation of acetate of potash, cannot be alleged; for this salt is not only soluble in alcohol, but is a mixture of alcohol and acetic acid.

M. Braconnot has added other observations to these, especially with respect to nitric acid. This acid, concentrated and boiling, does not at all act upon fragments of marble, or carbonate of barytes in powder; this non-action is attributed by him to the insolubility of the nitrates of lime and barytes in concentrated nitric acid, and to the affinity which retains the carbonic acid in its compounds.

M. Braconnot has also determined, in a manner which is apparently satisfactory, that if neither tin, iron, lead, nor silver, are attacked by concentrated nitric acid, it is because the nitrates of these metals are insoluble in this acid. It is to the same cause that he attributes all the results obtained by M. Pelouze.

The following new facts, which admit of the explanation of M. Braconnot in certain cases, cannot I think be generalized, and that other causes besides those mentioned oppose the action of acids upon bases or their carbonates.

One of the most remarkable chemical reactions is that which results from the contact of sulphuric acid with barytes. It is well known that this combination is sometimes effected with the extrication of so much heat, that the mass of barytes becomes red hot, and part of the sulphuric acid escapes in the state of vapour. I have found some peculiarities respecting this combination, which appear to me to possess some scientific interest.

A. A fragment of barytes, put into contact with cold Nordhausen fuming sulphuric acid, occasioned immediate and very vivid action. This action was still more vivid when anhydrous sulphuric acid, liquefied at about 77° Fahr., was employed.

B. A fragment of barytes, recently calcined, put into cold sulphuric acid, containing only one atom of water, of sp. gr. 1.848, suffered no alteration; no appearance of combination occurred. After remaining some time in contact, action suddenly takes place when the mixture is exposed to moist air; it may be also effected by slightly touching the barytes, moistened with sulphuric acid, with a hot iron, or a glass rod moistened with water.

C. A fragment of barytes was put into contact with sulphuric acid of sp. gr. 1.848, to which a small quantity of water was previously added, and incandescence was the immediate result. The action is equally speedy when weaker sulphuric acid is employed, but no incandescence occurs.

D. Sulphuric acid of density 1.848, which did not act upon recently calcined barytes, acted energetically upon barytes which had absorbed a little moisture from the air.

E. Hydrated sulphuric acid, properly diluted so as to act immediately on barytes, does not act when cold, if it is mixed with absolute alcohol or pyroxilic spirit.

From these different results, it may be inferred that hydrated sulphuric acid, containing only one atom of water, is with difficulty separated from it; it neutralizes in some mode the properties of the acid; for even in the presence of so powerful a base as barytes, the acid does not act without the assistance of heat.*

It becomes very important to state exactly the density of sulphuric acid, when it is employed in chemical reactions; for by the experiments above detailed, it appears that this acid combines energetically with barytes, when put into contact with this base at common temperatures, in the state of anhydrous acid, fuming acid, or when weaker than 1.848, but it ceases to act when it is exactly 1.848.

If the anhydrous acid, or the fuming acid of Nordhausen, did not combine with barytes very energetically, it might be inferred, in order to explain the necessity of weakening the acid of 1.848, that the formation of sulphate of barytes cannot occur under these cir-

* See Prof. Graham's paper on water as a constituent of salts in Lond. and Edinb. Phil. Mag. vol. vi. p. 327.—Edit.

cumstances, except by the previous formation of hydrate of barytes at the expense of a part of the water feebly retained by the sulphuric acid; but the facts stated render this opinion inadmissible. When employing acid of 1·848 density, the heat, as well as the addition of a little water, occasions the reaction, and in the latter case, the presence of the water unquestionably does not intervene, except by the extrication of the requisite heat. This extrication may be owing to different causes; in the experiment C, it may be attributed to the combination of a part of the water of the weak acid with barytes, or the formation of hydrate of barytes; and in the experiment D, it is hydrate of barytes ready formed, which, being more favourable to combination, gives immediate rise to the production of sulphate of barytes by its contact with sulphuric acid of density 1·848.

The explanations given by M. Braconnot of the non-action, under certain circumstances, of the acids upon metals, their bases or carbonates, are not, in the opinion of M. Lassaigne, applicable to the results of the experiments related; they are equally unsatisfactory in explaining the phenomena observed by Proust, and which relate to the action of nitric acid upon tin—an action which gives rise to the production of a compound which is insoluble (stannic acid), even when the acid is in the state most favourable for energetic action. He is also of opinion that in all the reactions described by MM. Proust, Pelouze, and Braconnot, the great stability of the compounds of acids and water, when they exist in the proportions stated with respect to the weights of their atoms, exerts great influence; and that the mixture of alcohol and æther with the acids results, not only from giving a liquid which is not susceptible of dissolving the product which may arise from the reaction of these acids on the bases or the carbonates, but from preventing all action from occurring by taking from the acids the portions of water which are not retained by stability of combination. The experiment E. gives support to this opinion.

In the contact of nitric acids with the metals, the presence of a little uncombined water also undoubtedly intervenes to facilitate the reaction. Ammonia, the formation of which occurs with iron, zinc, and cadmium, as is the case with tin, favours this opinion; but this influence cannot be readily admitted with respect to lead, copper, and silver.

In the course of these experiments, M. Lassaigne found that the action of nitric acid upon the metals is always accompanied with the formation of more or less ammonia, according as the metals decompose water more or less readily. The metals which do not decompose water yield no traces of ammonia.

In operating upon potassium and sodium, nevertheless, he obtained no traces of nitrate of ammonia, which he attributes to the high temperature produced, and at which the nitrate of ammonia cannot exist: these experiments with the metals of the alkaline oxides are not free from danger, on account of the violent explosion which takes place at the moment of contact between them and the nitric acid.—*Ann. de Chimie*, lxxvii. 209.

ON SUGARS. BY M. PELIGOT.*

Common Sugar.—On repeating the analysis with all possible care, the author found that the formula long since adopted is that which best agrees with experiment: this formula is $C^{24} H^{22} O^{11} \dagger$.

M. Berzelius found that the compound of sugar with oxide of lead, is $C^{24} H^{30} O^{10}$, $2PbO$. On drying this salt at 320° Fahr., M. Peligot obtained $C^{24} H^{18} O^9$, $2PbO$. Anhydrous sugar will therefore be $C^{24} H^{18} O^9$ instead of $C^{24} H^{30} O^{10}$.

The author also obtained a crystallized saccharate of barytes, crystallized by the direct contact of sugar and barytes dissolved in water, and he found the formula to be $C^{24} H^{22} O^{11}$, BaO ; he combined sugar with common salt, and found the composition of this body to be $C^{48} H^{42} O^{21}$, $Ch^2 Na$.

Sugar of Starch and of Diabetes.—The formula of these and of the grape and honey sugar, M. Peligot found to be $C^{24} H^{28} O^{14}$. He also analysed the compound of diabetic sugar and common salt obtained by Calloud, and found that this curious product is represented by the formula $C^{48} H^{52} O^{26}$, $Ch^2 Na$; the compound of sugar of starch and oxide of lead, obtained by the contact of ammoniacal acetate of lead and sugar dissolved in excess, was found to be $C^{48} H^{42} O^2$, $6PbO$; the saccharate of barytes from sugar of starch is represented by $C^{48} H^{56} O^{28}$, $3BaO$.

M. Peligot found that common sugar is the only one which combines with the alkalis without suffering change. Sugar of starch and all other known sugars, at first combine with the alkalis, and are gradually destroyed, giving rise to two distinct products, according to the circumstances of the mixture of these bodies.

Lime dissolved in a solution of starch sugar, gradually loses its caustic property, and is saturated by an acid formed by its influence. The salt of lime formed, when rendered neutral, is abundantly precipitated by subacetate of lead. The formula of the insoluble salt is $C^{48} H^{30} O^{15}$, $6PbO$. The disengaged acid could not be conveniently examined: it is not volatile, and forms salts, almost all of which are soluble in water. On heating the solution of starch sugar and an alkali, a more rapid action is observable; the mixture becomes coloured, and a brownish-black acid is formed, having some resemblance to ulmic acid, but it is quite distinct from it. Its composition is represented by the formula $C^{18} H^{30} O^{10}$. It appears to be identical with the acid obtained by M. Svanberg, in treating the acid of catechu with caustic potash, which has the composition represented by the preceding formula; nevertheless differences occur in the analyses, which indicated one per cent. too much hydrogen. This acid is very readily obtained with fused starch sugar, and a concentrated solution of potash; the action is rapid. When the colour has become very intense, water is added, and the acid is precipitated by hydrochloric acid. If it be identical with the japonic acid, this acid is represented by $C^{46} H^{16} O^8$. These two acids differ from sugar

* See Prof. Graham's paper, p. 219. of the present number.—EDIT.

† The original atomic weights are preserved.

only in being minus water: for, $C^{48}H^{42}O^{34}$ anhydrous sugar, becomes $C^{48}H^{30}O^{18}$, the first acid, by losing $6H^2O$; then $C^{48}H^{30}O^{15}$ becomes $C^{48}H^{16}O^8$, japonic acid by losing $7H^2O$. Sugar thus loses water successively even in the midst of water. This remarkable transformation is well characterized with starch sugar, and analogous sugars. When the sugar and alkali are not in contact with water, the phenomena of decomposition no longer occur: an alkaline saccharate is obtained in which the sugar possesses its usual properties.

M. Peligot has examined the nature of the action of acids, and particularly that of concentrated sulphuric acid upon sugars. With sulphuric acid and common sugar, a deep colour is produced, and a certain quantity of japonic acid is formed. With sugar of starch, on the contrary, there is no colour; and what is very remarkable is, that this sugar and the acid combine and form sulphosaccharic acid. This is to be saturated with carbonate of barytes, and treated with subacetate of lead: sulphosaccharate of lead is precipitated, the composition of which is $C^{48}H^{40}O^{30}SO^3 + 4PbO$; but it has not been precisely determined what quantity of water the sulphosaccharic acid contains. This acid when uncombined is not very stable: it does not precipitate barytic salts, and in general forms soluble salts.

The action of heat upon sugars, when properly managed, yields very simple results; at about 410° Fahr., water only is obtained, and a black product remains, which is entirely soluble in water. The author has preserved the name of *caramel* for it. When purified by alcohol a tasteless substance is obtained, which does not ferment. Its composition is very simple, $C^{48}H^{56}O^{18}$; and it differs only from sugar in losing a part of its water. Common and starch sugar, treated in this way, both yield the same substance.

These experiments, it will be seen, greatly modify the present opinions of the atomic weight of sugars, confirming the analyses already made of cane and starch sugar.

Journal de Ch. Med.—June, 1838.

SUCCISTERIN.

MM. Pelletier and Walter, in examining the pyrogenous products of amber, have obtained and analysed several substances, among which there is one that they think worthy of being particularly noticed.

It is white, crystalline, scarcely soluble in alcohol or æther, and its colour is rendered intensely blue by sulphuric acid. The analysis which they have performed indicates the formula C^3H^1 ; it has therefore the same composition as idrialin, and possesses also all its properties. It is well known that idrialin, which was discovered by M. Dumas, has been met with only in a mineral, the site of which is lost, and is found only in a few mineralogical collections. The authors do not assert the identity of idrialin with the substance which they have found in amber. If it be supposed that they are merely isomeric, they propose the name of *succisterin* for the newer compound.—*L'Institut*, Juin, 1838.

Register of Meteorological Observations for June 1838, made at Applegarth Manse, Dumfries-shire. By the Rev. Wm. Dunbar.

(Omitted last Month.)

| Days of Month. | Barometer. | | Therm. | | Wind. | Rain. | Weather. |
|----------------|------------|--------|--------|--------|----------|-------|---|
| | 9 a.m. | 9 p.m. | 9 a.m. | 9 p.m. | | | |
| June 1 | 29.80 | 29.75 | 50 | 49 | E. | ... | Cold and withering. |
| 2 | 29.63 | 29.58 | 49½ | 49 | SW. | ... | Fine: rain: genial. |
| 3 | 29.54 | 29.50 | 55 | 52 | SSW. | ... | Showery and sunny. |
| 4 | 29.49 | 29.60 | 59 | 53 | S. | ... | Showery and warm. |
| 5 | 29.71 | 29.85 | 62 | 52½ | S.W. | ... | Showery: thunder. |
| 6 | 29.96 | 30.02 | 61½ | 47 | E. by N. | ... | Showery: cold P.M. |
| 7 | 30.05 | 30.08 | 56 | 45½ | SSW. | ... | Dry: cool: genial. |
| 8 | 30.13 | 30.11 | 55 | 48½ | N. & S. | 1.12 | Dry: cool: genial. |
| 9 | 29.97 | 29.71 | 54 | 50 | SSW. | ... | Dry, but threatening rain. |
| 10 | 29.48 | 29.40 | 50 | 51 | S. | 0.46 | Wet all day. |
| 11 | 29.49 | 29.50 | 56 | 49 | SE. | 0.72 | Wet: thunder. |
| 12 | 29.61 | 29.67 | 55 | 49½ | NE. | ... | Dry and pleasant. |
| 13 | 29.68 | 29.64 | 53 | 51 | NE. | ... | Dry: rather cool. |
| 14 | 29.51 | 29.50 | 51 | 48 | NE. | 0.62 | Wet throughout. |
| 15 | 29.50 | 29.49 | 53 | 53 | SW. | 1.80 | Very genial day. |
| 16 | 29.53 | 29.55 | 55 | 55½ | S. | 1.46 | Soft rain all day. |
| 17 | 29.64 | 29.67 | 57 | 56 | S. by W. | ... | Fine growing weather. |
| 18 | 29.55 | 29.36 | 60 | 57 | N.E. | ... | Fair A.M.: wet P.M.: thund. |
| 19 | 29.30 | 29.50 | 55½ | 55 | W. | 0.96 | Wet preced ^d night: dry P.M. |
| 20 | 29.49 | 29.20 | 59 | 50 | SW. | 1.32 | Very wet afternoon. |
| 21 | 29.21 | 29.25 | 57 | 51 | SW. | 3.74 | Fair all day. |
| 22 | 29.37 | 29.60 | 52 | 52½ | SW. | ... | Showery A.M.: cleared. |
| 23 | 29.89 | 29.91 | 60 | 54½ | SSW. | ... | Fair: fine day. |
| 24 | 29.80 | 29.80 | 62 | 56 | SSW. | ... | Fair all day. |
| 25 | 29.91 | 29.93 | 56 | 58 | SSW. | ... | Fair and mild. |
| 26 | 29.86 | 29.86 | 66 | ... | SE. | ... | Fair and warm. |
| 27 | 29.75 | 29.80 | 56 | 50 | SE. | ... | Wet A.M.: cleared up. |
| 28 | 29.80 | 29.85 | 61 | 48½ | S. | ... | Fine summer day. |
| 29 | 29.87 | 29.85 | 54 | 54 | NE. | ... | Showery, but warm. |
| 30 | 29.70 | 29.74 | 54½ | 56 | NE. | 0.60 | Cloudy and moist. |
| Mean. | 29.71 | 29.67 | 56 | 51½ | | 7.26 | |

METEOROLOGICAL OBSERVATIONS FOR JULY 1838.

Chiswick.—July 1. Cloudy and fine: rain. 2. Sultry: rain. 3. Rain. 4. Hazy: fine. 5. Very fine. 6. Heavy rain with thunder: fine. 7, 8. Fine. 9—11. Very fine. 12. Overcast. 13. Very hot: lightning at night. 14. Rain. 15. Showery. 16—22. Very fine. 23. Overcast. 24, 25. Fine. 26. Cloudy and fine: rain. 27. Fine. 28. Very fine: slight rain. 29. Cloudy: rain. 30. Heavy showers. 31. Very fine.

Boston.—July 1. Cloudy: rain early A.M. 2. Cloudy: rain P.M. 3, 4. Cloudy. 5. Fine. 6. Fine: rain P.M. 7. Fine. 8. Rain. 9. Cloudy. 10. Fine. 11. Cloudy: rain early A.M.: rain P.M. 12. Cloudy. 13. Fine: thunder and lightning P.M. 14. Cloudy: rain early A.M.: rain P.M. 15. Fine: rain P.M. 16. Cloudy. 17. Fine. 18. Cloudy. 19. Fine. 20—22. Windy. 23. Fine: rain P.M. 24. Fine. 25. Cloudy. 26. Fine: rain P.M. 27. Stormy. 28. Fine. 29. Fine: rain A.M. 30. Fine: rain A.M. 31. Fine.

Applegarth Manse, Dumfries-shire.—July 1. Shower A.M.: fair P.M. 2. Fair all day. 3, 4. Fine summer days. 5. Excellent weather. 6. Warm: thunder: rain. 7. Showery all day. 8. Fair: mild: cool P.M. 9. Dull day: very cloudy. 10. Rain in the afternoon. 11. Rainy all day: fog P.M. 12. Rain: cleared up P.M. 13. Showery. 14. Showery all day. 15. Showery: cleared P.M. 16. Showery A.M.: cleared. 17. Wet all day. 18. Fine day: moist P.M. 19. Showery all day. 20. Fair day, though cool. 21. Fair A.M.: showery P.M. 22. Fair throughout. 23. Heavy rain: thunder. 24. Fair throughout. 25. Fair, but cool. 26. Wet nearly all day. 27. Showery A.M. 28. Showery nearly all day. 29, 30. Showery P.M. 31. Fair throughout.

| Days of Month.
1838.
July. | Barometer. | | | Thermometer. | | | | | | Wind. | | | | Rain. | | | Dew-point.
9 a.m. |
|----------------------------------|--------------------------------|-----------|--------|--------------------|---------------------------|------------------|-------------------|------|-----------|-------|---------------------------|--------------------------------|-----------|---------|-----------------|------|----------------------|
| | London:
Roy. Soc.
9 a.m. | Chilwick. | | Boston.
8½ a.m. | Dumfries-shire.
9 a.m. | | London: Roy. Soc. | | Chilwick. | | Dumfries-shire.
9 a.m. | London:
Roy. Soc.
9 a.m. | Chilwick. | Boston. | Dumfries-shire. | | |
| | | Max. | Min. | | Fahr. 9 a.m. | Self-reg. 9 a.m. | Max. | Min. | Max. | Min. | | | | | | | |
| 1. | 29.928 | 29.980 | 29.913 | 29.40 | 29.79 | 29.90 | 62.2 | 67.0 | 55.5 | 76 | 56 | 60 | SE. | S. | SE. | SE. | 57 |
| 2. | 30.048 | 30.031 | 29.992 | 29.53 | 29.96 | 29.96 | 61.8 | 65.7 | 57.3 | 78 | 56 | 59 | E. | S. | E. | SE. | 59 |
| 3. | 30.070 | 30.065 | 30.025 | 29.50 | 29.96 | 29.98 | 63.6 | 71.4 | 59.7 | 70 | 51 | 62 | NW. | SW. | NW. | SE. | 60 |
| 4. | 30.140 | 30.142 | 30.121 | 29.52 | 29.98 | 29.98 | 65.5 | 68.0 | 58.2 | 80 | 48 | 69 | S. | S. | S. | E. | 59 |
| 5. | 30.198 | 30.166 | 30.080 | 29.57 | 29.98 | 29.98 | 67.8 | 71.4 | 57.6 | 81 | 49 | 68 | E. | SE. | SE. | SE. | 60 |
| 6. | 30.050 | 30.042 | 29.996 | 29.43 | 29.90 | 29.70 | 64.0 | 74.0 | 60.2 | 77 | 52 | 71 | SE. | S. | S. | SE. | 63 |
| 7. | 30.010 | 29.997 | 29.972 | 29.33 | 29.65 | 29.65 | 64.3 | 73.2 | 56.7 | 70 | 54 | 64 | SE. | SW. | SW. | SE. | 59 |
| 8. | 29.968 | 30.077 | 29.995 | 29.30 | 29.70 | 29.83 | 62.7 | 69.6 | 58.0 | 72 | 56 | 60 | SW. | W. | W. | SW. | 59 |
| 9. | 30.182 | 30.164 | 30.149 | 29.50 | 29.83 | 29.86 | 66.2 | 70.6 | 59.2 | 78 | 56 | 64 | W. | W. | W. | SW. | 60 |
| 10. | 30.204 | 30.188 | 30.130 | 29.46 | 29.79 | 29.77 | 65.2 | 74.6 | 59.2 | 80 | 60 | 64 | SW. | SW. | SW. | SW. | 61 |
| 11. | 30.160 | 30.115 | 30.042 | 29.40 | 29.79 | 29.72 | 65.2 | 74.3 | 59.3 | 80 | 59 | 71 | SW. | SW. | SW. | S. | 61 |
| 12. | 30.100 | 30.077 | 29.994 | 29.35 | 29.72 | 29.80 | 67.4 | 78.2 | 61.4 | 82 | 60 | 68 | S. | SW. | SW. | S. | 63 |
| 13. | 30.030 | 30.006 | 29.803 | 29.35 | 29.74 | 29.58 | 69.7 | 73.6 | 60.7 | 84 | 58 | 68 | W. | SW. | SW. | S. | 62 |
| 14. | 29.766 | 29.752 | 29.669 | 29.06 | 29.43 | 29.19 | 66.3 | 78.8 | 62.5 | 84 | 56 | 66 | SW. | SW. | SW. | SW. | 63 |
| 15. | 29.762 | 29.905 | 29.722 | 29.50 | 29.27 | 29.50 | 65.7 | 68.0 | 59.0 | 73 | 51 | 66 | SW. | SW. | SW. | SW. | 60 |
| 16. | 30.096 | 30.202 | 30.024 | 29.40 | 29.65 | 29.90 | 65.2 | 76.7 | 55.3 | 75 | 45 | 65 | NW. | SW. | NW. | W. | 61 |
| 17. | 30.252 | 30.221 | 29.958 | 29.53 | 29.94 | 29.78 | 65.9 | 79.0 | 54.3 | 74 | 57 | 64 | S. | SW. | SW. | NW. | 59 |
| 18. | 30.116 | 30.283 | 30.100 | 29.49 | 30.00 | 30.06 | 63.8 | 73.5 | 60.0 | 73 | 51 | 61 | NW. | W. | NW. | NW. | 60 |
| 19. | 30.266 | 30.265 | 30.057 | 29.63 | 29.90 | 29.80 | 66.8 | 72.8 | 57.2 | 78 | 50 | 66 | S. | W. | NW. | SW. | 59 |
| 20. | 30.012 | 30.007 | 29.983 | 29.33 | 29.75 | 29.80 | 62.0 | 74.3 | 55.8 | 75 | 49 | 65 | W. | W. | NW. | NW. | 60 |
| 21. | 30.050 | 30.132 | 30.031 | 29.48 | 29.97 | 30.06 | 59.4 | 71.3 | 53.2 | 67 | 51 | 58 | N. | N. | N. | NW. | 55 |
| 22. | 30.200 | 30.188 | 29.160 | 29.64 | 30.10 | 30.05 | 56.0 | 68.4 | 52.3 | 64 | 42 | 57 | NW. | NW. | NW. | NW. | 52 |
| 23. | 30.184 | 30.182 | 29.986 | 29.66 | 29.99 | 29.99 | 58.4 | 61.8 | 50.4 | 70 | 52 | 57 | NW. | NW. | NW. | NW. | 53 |
| 24. | 29.988 | 30.002 | 29.970 | 29.44 | 29.88 | 29.88 | 56.4 | 63.3 | 53.8 | 67 | 40 | 56 | NW. | NW. | NW. | NW. | 50 |
| 25. | 30.008 | 30.008 | 29.998 | 29.48 | 29.88 | 29.83 | 58.8 | 62.8 | 49.6 | 70 | 41 | 56 | W. | NW. | NW. | NW. | 53 |
| 26. | 29.982 | 29.992 | 29.708 | 29.38 | 29.70 | 29.55 | 60.7 | 65.5 | 49.7 | 70 | 51 | 59 | NW. | NW. | NW. | NW. | 51 |
| 27. | 29.820 | 29.830 | 29.804 | 29.15 | 29.57 | 29.55 | 60.0 | 66.7 | 52.7 | 71 | 48 | 59 | W. | NW. | NW. | NW. | 54 |
| 28. | 29.822 | 29.816 | 29.711 | 29.21 | 29.52 | 29.43 | 61.8 | 70.4 | 53.3 | 72 | 50 | 61 | SW. | SW. | NW. | S. | 54 |
| 29. | 29.648 | 29.651 | 29.620 | 29.07 | 29.40 | 29.40 | 60.7 | 68.3 | 53.3 | 72 | 44 | 61 | W. | W. | NW. | SW. | 56 |
| 30. | 29.708 | 29.698 | 29.792 | 29.11 | 29.60 | 29.50 | 61.4 | 71.3 | 50.2 | 72 | 44 | 58 | SW. | W. | SW. | SW. | 55 |
| 31. | 29.884 | 30.008 | 29.861 | 29.29 | 29.60 | 29.77 | 61.7 | 70.5 | 50.0 | 74 | 47 | 60 | W. | W. | SW. | NW. | 55 |
| Mean. | 30.022 | 30.038 | 29.947 | 29.40 | 29.44 | 29.44 | 63.3 | 70.8 | 56.0 | 74.48 | 50.77 | 62.7 | Sum. | 2.19 | 1.64 | 4.93 | Mean. |

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THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1838.

XXXIII. *Remarks on the Constitution of the Atmosphere; addressed to Dr. Dalton, F.R.S., by JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in Hampden Sydney College, Virginia, Member of Acad. Nat. Sc. Philad., &c. &c.**

Hampden Sydney College, Virginia, U. States,
RESPECTED SIR, June 25, 1838.

I HAVE this morning read, with much pleasure, the remarks inserted by you in the Numbers of the London and Edinburgh Philosophical Magazine for February and May last, in relation to the constitution of the atmosphere, and other important points connected with investigations of its composition.

It fully appears, if we admit the hypothesis brought forward many years ago by you, and generally received by chemists, that a gas acts as a vacuum to the particles of one of another kind, that the constitution of the atmosphere is not such as it ought to be. The hypothetical result, contrary to what is popularly imagined, would indicate a continuous variation in the composition of air at different altitudes, and give us two limits, the one marking out an elevation beyond which oxygen could not be found, and the other the same for the azote. From a long experience in these matters, and a perfect acquaintance with these theories, you have given it as your opinion, that in the higher regions of the air, the proportion of oxygen to azote is less than at the surface of the earth, but not nearly so much as the theory of mixed gases would require, and that the reasons for this must be found in the incessant agitation of the atmosphere from winds and other causes.

In America, partly for the foregoing reason, and partly for

* Communicated by the Author.

Phil. Mag. S. 3. Vol. 13. No. 82. Oct. 1838.

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others to which I will presently allude, some chemists have altogether rejected the hypothesis of gaseous action, perhaps on very insufficient grounds. It is therefore desirable that the subject should be more extensively investigated, and these objections set aside. No one can do this more ably than yourself.

If a soap-bubble be expanded with hydrogen gas, in an atmosphere of common air, and then be suddenly burst, so as to accomplish an instantaneous diffusion and intermixture, at the moment at which this occurs there is an expansion, which is apparently of a thermal kind, inasmuch as the gaseous mixture, in a short space of time, recedes to its original volume. One hundred measures of hydrogen and four hundred of atmospheric air occupy, on the moment of being mixed in this way, considerably more space than five hundred measures. Does not this indicate that the particles of these gases occupy, when their temperature has fallen to the original degree, a less space than the sum of their volumes before mixing? Is it not a phenomenon of the same kind as that observed on mixing alcohol and water, when there is a thermal disturbance, followed by a penetration of dimensions? In other words, does not this experiment give indications of proof, that certain gases, on being *simply mixed*, exist in a condensed state?

This result is readily observed on mixing hydrogen with atmospheric air, and also with nitrogen gas. I detected it some years ago, but have not yet been able to show it in the case of other gases. If it be really due to a condensation taking place, it is an experiment of no ordinary importance; especially if it should be found that the same occurs on mixing oxygen with nitrogen. It would indicate to us one of those "other causes" which keep up the integrity of the constitution of the atmosphere.

One of the most powerful arguments brought forward in support of the hypothesis of gaseous action, is founded on the experiments of Professor Graham; it is, that the law which regulates the flow of gases into a vacuum, is precisely the same as that which regulates their flow into each other: is this however strictly the case?

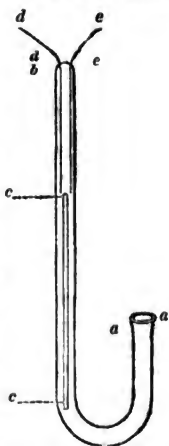
Professor Graham has shown*, that when hydrogen gas, carbonic acid, &c. are separated from atmospheric air by a thin screen of stucco, they diffuse themselves according to the law of the square roots of their density. One volume of air, for example, replaces 0.8091 of carbonic acid gas; the gas therefore on that side of the screen where the carbonic acid was placed,

[* See L. and E. Phil. Mag. vol. ii. p. 175; vol. iv. p. 321.]

increases in quantity. But if any one will throw into a soap-bubble one hundred measures of carbonic acid, and expose it to atmospheric air, he will perceive a very different result to that just mentioned, for instead of gas accumulating within the bubble, a very extraordinary and *rapid diminution* will ensue: this phænomenon is not a little remarkable; it does not require any instrumental arrangement to detect it. A bubble of carbonic acid gas an inch in diameter, collapses in the space of a few minutes to the size of a common pea.

Again, if carbonic acid, &c. and atmospheric air are kept from directly mingling with each other, by being separated by a thin lamina of India-rubber, they will pass through the barrier to intermix. Do they intermix with a force greater than the pressure of *one* atmosphere? Dr. Mitchell of Philadelphia found that carbonic acid would pass through a piece of India-rubber, and diffuse itself into atmospheric air, though resisted by a pressure greater than *two* atmospheres (63 inches of mercury). This first cast doubts on the hypothesis of gaseous action, for if it can be proved that these mixtures are effected with a force greater than that which is measured by one atmosphere, the idea that gases act towards each other as *vacua*, necessarily falls to the ground.

Allow me, Sir, to point out some experiments which seem to bear on this matter. Let us examine, for instance, whether sulphurous acid will pass into atmospheric air with a force greater than the pressure of one atmosphere. A tube of glass, about one third of an inch in bore, and ten inches long, is bent into a kind of siphon so that one leg shall be about six, and the other two inches long. The extremity *aa*, has a lip or rim turned on it, at the lamp; and in the longer leg a thin glass tube *cc*, about one eighth of an inch in bore, and closed at one extremity, is included to serve, as will be hereafter shown, as a gauge. Next, the extremity *b* of the siphon is closed, there being inserted through it two platinum wires *dd*, *ee*, parallel to each other, but not touching. The arrangement is thus completed for use. Let us suppose it is required to pass through India-rubber, sulphurous acid gas, into atmospheric air condensed by a pressure of five or six atmospheres; the long leg of the siphon is to be filled with water, which is excluded from the gauge tube *cc*, owing to its narrowness; next, a strong decoction of litmus is to be placed in the short leg, until it is half



filled. The rim round the extremity *a a*, is then daubed with a piece of burnt caoutchouc, and upon it is tied a thin piece of that substance, with a fine but strong waxed thread. Over this is secured a piece of stout silk or cotton cloth, for the purpose of fortifying the elastic barrier. The wires *d d e e*, are next made to communicate with the poles of an active voltaic battery, and the condensation commences; for the gas which is evolved from these electrodes, rising to the top of the tube, accumulates there, causing the column of water in the short leg to rise and condense the atmospheric air above it. The membrane though fortified gives way to a certain extent, becoming convex outwards; and as the accumulation of gas in the long leg continues, the condensation of that in the short one increases, as is indicated by the gauge *c c*. A very thin India-rubber, of the diameter here indicated, will stand a pressure of 6 to 20 atmospheres without rupture, if its silken support is good; and I have found that anointing the edges of the rim with the burnt substance enables the operator to tie the barrier on so that no leakage can occur between it and the glass, under the severest pressures. When the gauge indicates that the required degree of condensation is arrived at, the connexion with the battery is broken, and the condensation of course stops: the siphon being carried to the mercurial trough, taking care to keep its position erect, its short limb is depressed under the mercury and carried into a jar containing sulphurous acid. If, under these pressures, any of the acid gas finds its way into the condensed air, its presence is detected by the reddening of the blue litmus water. It is necessary here to observe, that the indications of the tube gauge do not give a correct estimate of the amount of condensation, but always represent them higher than they are, according to Marriotte's law. It has long been known, that the volume of gas dissolved in water depends in a great measure on the pressure exerted on it; now it will be found, when the operation is conducted in an instrument arranged as this, that a certain proportion of the air in the gauge disappears in this manner. Its zero point is therefore altered, and the condensation appears higher than it really is. It may be remarked, in passing, that it is surprising to see to what an extent the absorption of the oxygen and hydrogen is carried in the longer leg, owing to their making their appearance in the nascent form. To ascertain the true condensation, so soon as the passage of the sulphurous acid or other gas has taken place satisfactorily, the membrane is to be punctured with a pin; and when a pneumatic equilibrium is attained, the height of the liquid in the gauge will mark the point where the zero of the scale should be placed.

In this way it may be shown, that sulphurous acid will pass instantaneously into atmospheric air, against a pressure equivalent to two hundred and twenty inches of mercury, or *seven atmospheres and a third*.

The curved form of the instrument just described was found to present certain inconveniences when pressures upwards of 6 or 7 atmospheres were made use of. The volume of air, which at the beginning of the experiment occupied the greater part of the extent of the shorter limb, had now collapsed much in its dimensions, and owing to the unavoidable giving way of the India-rubber and silk cover, had retreated beneath it out of sight. It was not found expedient to lengthen this limb, for that entailed a corresponding increase in the dimensions of the battery, in order to produce a given condensation in a given time. A straight tube was therefore taken, about three sevenths of an inch in bore, and a rim turned on it at *aa*; at the closed extremity the platina wires *bc* entered; a gauge tube *d* was dropped in between them; water was then poured to the height *ee*; and lastly, a tube *f*, containing an appropriate chemical test, was inserted, its bottom resting on the top of the gauge tube. Nothing remained but to tie on the India-rubber with its silken support, and by the voltaic battery to proceed to condense. In this instrument the test fluid was never out of sight, nor did the volume of the gas suffer any inconvenient change; the gauge too was well located for observation, and a given condensation could be produced in less time, and by a less amount of electricity, than with the siphon tube. It is to be observed, however, that the gaseous matter evolved from the water mingles with the atmospheric air in the upper part of the tube, and therefore the passage of the gases tried, does not take place into atmospheric air, but into a mixture of oxygen, hydrogen, and nitrogen.



The tube *f* being filled with lime water, and a pressure amounting to *ten atmospheres* being produced in the vessel, it was exposed to an atmosphere of carbonic acid gas, at ordinary pressures. In the course of a few minutes, the upper part of the tube containing the lime-water began to look milky, and in an hour a cloud of particles of carbonate of lime had fallen to the bottom.

Again, having filled the test tube *f*, with a solution of acetate of lead, and produced a pressure amounting to *twelve*

atmospheres, it was exposed to sulphuretted hydrogen. In a very short time the black sulphuret of lead appeared, giving tokens of the rapid passage of the gas. A comparative experiment was made, in order to discover whether the transmission took place more slowly than when it was resisted by such a severe pressure. It appeared, however, so far as the experiment could be tried under similar circumstances, as regards the thickness of the barrier, &c., that sulphuretted hydrogen gas went through a barrier against a pressure of *three hundred and sixty inches of mercury*, to mix with another gas, as readily as if no force were exerted against it.

As numerous experiments, which had been tried on various gases, had failed to indicate any obstacle to their passage, it became necessary to know whether at the most extreme pressures that could be commanded they would pass through a barrier. To accomplish this, I took a strong and narrow tube, and having turned a rim at one end, and sealed five platina wires into the other, I *filled* it with distilled water, and inclosed a narrow capillary tube in it, the gaseous contents of which were small. As a test, in the upper part of the arrangement, and in lieu of the tube *f*, I placed a slip of paper which had been alternately soaked in acetate of lead and carbonate of soda; the India-rubber was fortified by a piece of very strong silk, which was carefully tied on; there was not therefore any gaseous matter present, except the small quantity of air in the gauge tube. The condensation went on with great rapidity, a mixture of oxygen and hydrogen gradually accumulating in the top of the vessel, bulging out the India-rubber and silk barrier, until it was almost hemispherical. It was my intention to try a pressure of twenty-five atmospheres; and when that was supposed to be reached, the instrument was immersed in sulphuretted hydrogen. Very soon the test paper became of a tawny yellow, and finally it was quite black; the pressure when the experiment was over was determined to be twenty-four and a quarter atmospheres.

At a temperature of 48° Fahr., and pressure 29.74 bar. sulphuretted hydrogen gas passes through a barrier into a mixture of oxygen and hydrogen, though it may be resisted by a pressure of *twenty-four and a quarter atmospheres*, or *nearly seven hundred and thirty inches of mercury*. Like sulphurous acid, it will become diffused into an atmosphere beyond it under a greater pressure than that which is sufficient to *condense it into a liquid*.

These results would appear, at first sight, entirely opposed to the hypothesis of gaseous action, and important enough to cast doubts upon its correctness, if not entirely to destroy it.

To me, however, it seems that an explanation can be given of them, which will lead us entirely to a different conclusion, and furnish a beautiful illustration of the truth of that hypothesis.

It appears there is abundant and conclusive evidence, that under ordinary circumstances of temperature and pressure, any given gas bears the same relation to one which is percolating into it that a vacuum would do, for the law of discharge is identically the same. For the purpose of illustration, we may therefore regard it to all intents as a vacuum, and reason accordingly. If the particles of heterogeneous gases possess no repulsive tendency as respects each other, but are perfectly quiescent and neutral, then it is immaterial how many of such particles are condensed together into a given space, for owing to the want of repulsive action in those particles, that space will be as much a vacuum to any other gas as it ever was. Now it has just been shown that certain gases will diffuse into others even though the latter may be condensed into a space twenty times less than that which they would ordinarily occupy. The vacuum is not the less a vacuum because it is contained under smaller dimensions, any more than a Torricellian vacuum is less perfect when the mercury is made to rise nearly to the top of the barometric tube, than it was when there was a vacant space many inches in length. Theory would therefore indicate, that these diffusions might take place under all pressures, provided the gaseous condition subsists.

Moreover, the foregoing experiments do not actually furnish any proof that gases diffuse themselves into one another with a force greater than one atmosphere. It is a mistake to adduce them as examples in point, for the fact is that the barrier or tissue, far from being passive, exerts a very remarkable action in virtue of its *absorbing power*, a property pre-eminently possessed by charcoal, and some other porous bodies. This seems to afford an explanation of the whole phænomenon, and furnish an important fact in a physiological point of view,—that membranes and tissues are occasionally the origin and seat of powers of uncommon intensity.

It will be convenient, for the better understanding of these actions, to consider them under two heads. First, where the barrier between the media exerts no absorbent action on the media; this will include most of the results of Prof. Graham: secondly, where one of the media is absorbed to a much greater extent than the other; this will include all the foregoing experiments.

In the first case, the velocities with which any two gases

pass into a vacuum are inversely proportional to the square roots of their densities respectively: moreover, the volumes that so pass vary directly as the velocities, and therefore may be taken as an index and measure of them; but as the mass of each gas is expressed by the product of its density into its volume, it may be represented by the velocity multiplied into the density; and as the square of the velocity of one gas multiplied into its density is equal to the square of the velocity of the other multiplied into its density, whatever may be the difference of the specific gravity of the two gases, their mechanical momentum will always be the same; the resistance they meet with in passing through the tissue is common to both, and equal in both cases, and hence the initial velocities of diffusion ought to be inversely proportional to the square roots of the densities; and as during the progress of the experiment the impelling force of the one gas is equal to the expelling force of the other, the resulting momenta of the two currents is still equal, and the final volumes are such as are found by direct experiment.

In the second case. We have first to refer here to a fundamental proposition of dynamics, that when the moving force and the matter to be moved vary in the same proportion, the resulting velocity will be the same. An illustration will show the application of this principle to the case in hand: if a cylinder of air, fitted appropriately with a piston, communicates with a vacuum by means of a narrow aperture, it is immaterial whether the air be allowed to flow into the void without any pressure, or whether it be urged by a direct action on the piston; its velocity as it goes into the void will in both cases be the same; for if it be compressed, the immediate action of the force exerted on the piston is to reduce the air in the cylinder to such a density that its elasticity shall be equivalent to the compressing force; and because the elasticity varies as the density, the density of the air will increase with the expelling force; the matter to be moved is therefore increased in the same proportion with the pressure, and the final velocity is therefore the same. Now what is here said of a cylinder of compressed air, applies evidently to the action of barriers, such as sheets of water or India-rubber, which are nothing more than perpetual and equable condensing engines. When one of these is employed, if it increases the elastic force of a gas by compressing it, at the same time it increases its density, and therefore the velocity of transit is the same as though the gas had suffered no action of compression.

Such is the case whilst the gases are engaged with each

other in the barrier, but as soon as they are passed from it and are beyond the reach of its attractive force, a new condition of things takes place; the condensed gas being no longer under restraint, expands freely into a void, and when there measured, gives a resulting volume totally different to what it would have given had not the tissue compressed it. Suppose, for example, we place on one side of a barrier carbonic acid gas, of which it could condense its own volume, and on the other atmospheric air on which it exerted no action. Whilst the two gases were engaged together in the barrier the one would be presented to the other under an elastic force double of that which it would have had, if no absorption had gone on; but since its density is directly proportional to its elastic force, the continual velocity with which it rushes into the other gas is the same as though no compression whatever had occurred; the rate of exchange in the barrier is the same as under normal circumstances, that is to say, every volume of air replaces 0.8091 of compressed carbonic acid; but so soon as this gas has reached the opposite side of the barrier and there escapes, its elastic force, being restrained by no compression, causes it to assume its original dimensions.

This explanation satisfies all the facts, and reduces these experiments to the operation of the hypothesis of gaseous action; I would not here be understood to say that there are no other disturbing actions going on in barriers except those that result from their absorbing power. A great disturbance often arises from the circumstance that when two gases are absorbed together they experience a greater condensation than each would in a separate state. It is therefore impossible to foretell what the result of diffusing one gas into another will be, by simply ascertaining how many volumes of either alone will be absorbed by the tissue, inasmuch as a greater or less condensation may happen when both are employed together.

Variations of temperature, which probably affect the power of absorption, and thereby the diffusive volumes, are experienced by all barriers. When charcoal, or any other porous mass, is placed in an atmosphere of gas which it can rapidly condense, its temperature rises, the effect apparently depending more on the velocity of absorption than on the final amount. In the case of ammonia, it does not even require a thermometer to discover the increase of temperature, for it is very sensible to the touch. On the other hand, when this condensed gas makes its escape, a corresponding diminution of temperature happens: it is immaterial by what means the

liberation of the gas is effected; the same result uniformly follows. If a porous mass, saturated with carbonic acid, be exposed to an atmosphere of hydrogen, it absorbs but a small quantity of this latter, whilst a very large amount of the former is liberated from its condensed state, and the thermometer indicates a fall of temperature; the resulting volume of the mixed gases being much larger than the original volume of hydrogen. And if a porous mass which has absorbed its due volume of hydrogen be immersed in an atmosphere of ammonia, the resulting volume of the mixed gases is much smaller than the original amount, and the porous mass becomes hot.

The observations here made on the vicissitudes of temperature which a porous mass experiences when successively immersed in an atmosphere of different kinds, obviously apply when the exposures instead of being consecutive are simultaneous. If, for example, a barrier separates carbonic acid and hydrogen gas, and absorbs the former to a large amount, but exerts little or no action on the latter, then the opposite sides of that barrier will be unequally heated. Suppose, for illustration, we call that surface of the barrier which looks towards the carbonic acid C, and the surface looking toward the hydrogen H; then because of the condensing action of the barrier on the acid gas, the surface C will become hot; but because this gas as soon as it has passed through the barrier expands, as into a void, when it reaches the surface H, that surface because of the expansion will become cold. We see, therefore, that immediately after the action of the barrier is first set up, the absorption of carbonic acid takes place on a hot surface, and its evolution from a cold one; whereas the absorption of the hydrogen takes place on a cold surface, and its evolution from a hot one. A modified result of course happens when both gases are absorbed in different degrees, and any prediction of the resulting action becomes a matter of much difficulty. When the barrier is very thin, or has a high conducting power as respects caloric, this distinct surface action may not rigidly occur, but the whole structure experiences some rise or diminution, a mean expressive of the condition of the two surfaces respectively.

On the 22nd of November 1837, on analysing atmospheric air at this place, there was found in it only 19.60 per cent. of oxygen, corroborative results being obtained by the use of deutoxide of nitrogen and hydrogen gas; but the day previous and the day following, the proportion was almost 21 per cent. This is not an insulated result; I know that on several occasions during the last four years the proportion of oxygen in the

air has varied in experiments that have been carefully made in Virginia. For a length of time these variations were imputed to the use of binoxide of nitrogen, and it was not until we made the same observations in using hydrogen gas, that the true cause was suspected. I believe that the binoxide of nitrogen will always give accurate results, if *added to atmospheric air*, in a stream of bubbles, by Hare's sliding rod eudiometer*: one fourth of the deficit is to be taken.

In the course of these investigations, it has happened to me to observe some instances of an action which you have so fully described. In a paper inserted in the American Journal of Medical Sciences for May 1836, it is remarked, "An important circumstance in gaseous analysis may here be noticed. If a tissue, in the act of transmitting gas or ready to do so, be placed in contact with another gas of a different nature, disturbance immediately ensues. A cubic inch of nitrogen made with phosphorus, but which was found to be contaminated with $4\frac{1}{2}$ per cent. of oxygen, was agitated briskly in a phial containing about an ounce of spring-water. In *one minute* the nitrogen gained one per cent. by the agitation. The same quantity of nitrogen agitated in a pint of water gained no less than *eleven* per cent. of oxygen. Nor is agitation or mechanical violence necessary to produce this important result. Into a bell filled with water and inverted into another vessel, so as not touch it in any point, I placed 100 measures of a gas, 85 of which were oxygen. After four weeks, an analysis was made, and the gas in the bell found to contain only 72 per cent. of oxygen, the remainder being nitrogen. In this way too, in the lapse of time, from an inverted vessel partially filled with atmospheric air, the oxygen will escape into the water, and thence into the atmosphere; and I have twice known this event to take place, so that the residue did not contain more than three or four per cent. of oxygen. In many of the most delicate researches of chemistry we have this disturbing cause in operation, which has for the most part been overlooked. Water is uniformly employed in our laboratories as a means of confining gases; it enters largely into our processes of pneumatic manipulation, and though we have hitherto neglected its action, it silently disturbs all our results. An air bell cannot pass to the top of a jar without instant contamination; during its residence there it is subject to a continued succession of changes; at no two moments is it the same in composition, a perfect freedom of communication existing between it and the atmosphere."

"As an instrument of rigid analysis, the pneumatic apparatus so arranged requires to be used with circumspection. It is

[* See Phil. Mag. and Annals, N. S., vol. vi. p. 114.]

impossible to keep oxygen, nitrogen, or any other gas in its original purity, if confined by water. This fluid, which when reduced to a thin imperceptible film is instantaneously permeated by almost every substance, undergoes the like action in course of time, even in deep masses. Gases are absorbed by it, and thrown off by it, in its purest state; how much more complicated then must its action be in that impure condition in which it is commonly used! Connected with this point, there is another: if a series of bells stand on a pneumatic trough, each will affect all the others, communicating a part of its contents and receiving from them in return. A jar containing binoxide of nitrogen, standing by the side of one containing common air, seriously affects it. I have noticed that two common tumblers, filled with these gases and so placed, communicate with each other, and so freely, that in 17 hours the tumbler originally filled with atmospheric air contained only $9\frac{3}{4}$ per cent. of oxygen. The habit of collecting gases at the same trough that is destined to preserve others is very exceptionable: we place the disturbing agency in circumstances the most favourable for its action. All operations of washing are liable to the same strictures."

I fear I have intruded upon you too long a letter. I have been encouraged to do so; for we are accustomed in America to associate with your name whatever there is of exactitude in chemistry. In gaseous mechanics the most important observations are due to your labours; and anything that may have even a remote bearing on the subject, will, I am persuaded, meet with acceptance from you.

Your obedient servant,

JOHN W. DRAPER.

XXXIV. *Observations on Shooting Stars on the Nights of the 9th, 10th, and 11th of August 1838.* By M. QUETELET, *Director of the Observatory of Brussels, &c. &c. &c.*, and by E. J. COOPER, *Esq. M.P.**

To Richard Taylor, Esq. Editor of the Philosophical Magazine.

DEAR SIR,

I RECEIVED the letter of which the following is an extract the day after the last meeting of the Physical Section of the British Association at Newcastle, and was therefore not able to comply with the request of M. Quetelet to present the interesting particulars which it contains to the notice of the Section. It seems to me that it I shall equally well effect

* See Lond. and Edinb. Phil. Mag., vol. xi. p. 261. Digitized by Google

M. Quetelet's objects by calling attention to his observations in your Journal, and by requesting that if any similar ones have been made in Great Britain, they may be communicated to you.

Very respectfully yours,

London, Aug. 30, 1838.

A. D. BACHE,
President of the Gir. Coll.
Philadelphia.

Extract of a Letter from M. Quetelet, Director of the Observatory of Brussels, to Prof. A. D. Bache, of Philadelphia.

"I observed the number of shooting stars visible here (at Brussels) on the nights of the 9th, 10th, and 11th of August. On the first of these nights the sky was almost entirely covered with clouds, and I saw but two shooting stars.

"On the night of the 11th, until nearly midnight, about one third of the sky towards the zenith was generally clear. Subsequently it was only clear at intervals, and towards two o'clock it rained. I was assisted in my observations by two other persons; and notwithstanding the unfavourable circumstances of the night we saw many shooting stars, besides several very brilliant meteors, with nearly all of them a luminous train. The uniform direction of their motion was further remarkable. Below is the result of our observations on the night of the 10th, giving the directions of the paths of the shooting stars and the hours between which the numbers given were seen.

49 directed from between the north and east towards the opposite quarter of the sky.

1 between the south and west towards the opposite quarter of the sky.

4 between the south and east towards the opposite quarter of the sky.

11 between the north and west towards the opposite quarter of the sky.

5 from east to west.

1 from west to east.

4 from north to south.

1 from south to north.

76

11 not well determined.

87 total number seen.

Of these were seen:

16 between 9—10 o'clock. The observations about 9 o'clock were interrupted for about 15 minutes by clouds.

29 between 10—11 o'clock.

39 between 11, and 11 and 50 minutes. The sky then clouded over.

3 after midnight in the clear intervals.

"I estimate at double the number just given what we should probably have seen had the weather been favourable.

"On the night of the 11th the sky was clear until towards two o'clock. At half past two it was covered with clouds. I had on this occasion three assistants. We saw

2 shooting stars before 9 o'clock.

34 between 9—10 o'clock.

19 between 10—11 "

24 between 11—12 "

32 between 12—1 "

12 between 1—2 "

10 between 2—3 "

2 after 3 o'clock.

135 total number of shooting stars which were seen.

The directions of the motions were

71 from between the north and east towards the opposite part of the sky.

3 between the south and west towards the opposite part of the sky.

16 between the south and east towards the opposite part of the sky.

10 between the north and west towards the opposite part of the sky.

10 from east to west.

2 from west to east.

13 from north to south.

6 from south to north.

131, leaving four, the directions of which were not well determined.

"The shooting stars of the 11th, though fine, were less so than those of the 10th. The general direction, as has been seen, was from the north-east towards the south-west."

[The following notice on the same subject has been communicated by the Rev. T. R. Robinson, D.D. of Armagh.]

Extract of a Letter from Edward J. Cooper, Esq., M.P.

"MY DEAR DR. ROBINSON, Geneva, Aug. 15, 1838.

"I have not been able to do much in arranging the observations made of falling stars on the nights 10—11 instant, as I only received those made by my companions last night.

The total number *seen* from 8^h to 16^h was upwards of 380. The number actually *observed* = 373, of which 123 fell to my list. My companions were M. Wartman and his two sons, M. Müller of the Observatory, and M. Borel, a friend of his. At 8^h two banks of clouds converged to a point on the horizon N. 40 E. This point moved subsequently about 5° more to the E.; and at 9^h 45^m the clouds had become much darker, and the angle of convergence had greatly increased. At 10^h they had become white and thin, and nearly concealed the north from the horizon to α Lyræ. About 14^h they had moved northward, and the point of convergence was at N. 35 E. Heavy dew, night perfectly calm. Three of the stars passed apparently over clouds. I can now merely class my own observations as follows. I find I am too late for post today, so will endeavour to class the total. Aug. 16th. I have found that it requires a very long time, so I can only give you the total numbers from and to each constellation. I shall place the number *from* before, and the number *to* after, the name of the constellation. I have not time to *check* my work.

| | |
|------------------------|---------------------|
| 16 Andromed. 6. | 1 Hon. Fred. 3. |
| 1 Antinoi 14. | 0 "Lizard" 1. |
| 0 Appar. Sculpt. 1. | 2 Libræ 5. |
| 10 Aquarii 6. | 0 Lyncis 4. |
| 16 Aquilæ 13. | 17 Lyræ 12. |
| 2 Arietis 2. | 0 Mont. Menal. 1. |
| 11 Aurigæ 7. | 1 Ophiuchi 9. |
| 18 Bootis 28. | 2 Orionis 4. |
| 6 Camelop. 2. | 16 Pegasi 15. |
| 2 Can. Ven. 5. | 17 Persei 13. |
| 5 Capricorn. 10. | 0 Pisc. Aust. 1. |
| 20 Cassiopeiæ 11. | 3 Piscis 2. |
| 128 Cephei 10. | 0 Ram. et Cerb. 3. |
| 1 "Cercle Murale" 1. | 4 Sagittæ 2. |
| 1 "Cervus Arcticus" 0. | 1 Sagittarii 6. |
| 5 Ceti 7. | 10 Serpentis 18. |
| 1 Custod. Messium 0. | 2 Scut. Sobiesk. 0. |
| 0 Comæ Ber. 1. | 2 Scorpionis 8. |
| 13 Coronæ Bor. 14. | 6 Tauri 6. |
| 26 Cygni 17. | 2 Taur. Poniat. 6. |
| 7 Delphini 0. | 0 Telescopii 1. |
| 40 Draconis 21. | 2 Trianguli 5. |
| 0 Equus Min. 1. | 31 Urs. Maj. 29. |
| 2 Gemini 6. | 22 Urs. Min. 21. |
| 21 Herculis 18. | 0 Virginis 1. |

"You will perceive that the numbers here particularized do

not accord with the grand total, nor can I, as I have already said, check them. I think however that there is enough to prove that on the 10th of August at least there has been nothing to support the idea of a common focus. I also must add, that on the following night there were only 3 per hour less in number."

XXXV. *Conjectures on the Cause of the peculiar Condition of Iron.* By Dr. SCHÖNBEIN.*

SEVERAL attempts have already been made to explain the peculiar condition of iron. Dr. Faraday's hypothesis upon that subject is certainly the most ingenious and most plausible of all the theories as yet brought forward, but there are some facts pointed out by me elsewhere, which do not well agree with the views of that distinguished philosopher. As the matter alluded to is of some importance with respect to electrical science, and all the theories hitherto announced upon the cause of the anomalous relations of the iron having failed, I may, perhaps, be allowed to suggest some ideas regarding the delicate point in question. But before doing it I must not omit to say, that I do not lay much stress upon my conjectures, and that I have only ventured to make them public, because I hope they will engage some philosopher more able than myself to take up the subject, and clear up by new investigations the darkness which still invests the cause of the inactivity of iron.

Chemistry in our days accounts for the difference of qualities exhibited by what are called "isomeric bodies" by asserting that the same number of particles of the same elements are capable of grouping themselves in different ways. Now if heterogeneous atoms be capable of combining in various manners, is it not possible that homogeneous ones may do the same, and in such a way as to give rise to substances essentially distinct from each other not only with regard to their physical, but also as to their chemical properties? Sulphur, selenium, phosphorus, and carbon prove indeed that simple substances are capable of assuming states almost as different from each other as any two isomeric bodies are in qualities. It is true, the difference of properties which we sometimes observe in the same element is generally referable to a modification of the cohesive attraction of its molecules brought about by the agency of heat; but I am almost sure,

* Communicated by Mr. Faraday, to whom it had been addressed by the Author.

that the chemical relations of a body are, more or less, modified also as often as a change of its cohesive state is effected. Sulphur for instance, in its peculiar (viscid) condition, phosphorus being liquid at the common temperature, selenium in its half fluid state, carbon as diamond, all these simple bodies are, most likely, in a chemical point of view, different from what they are in their usual condition. We certainly want as yet tests to ascertain the chemical difference which exists, for instance, between viscid and common sulphur, &c.; but though this be the case, analogy, I think, makes up for that want, and allows of our making an assertion of the kind. There are, indeed, many instances in chemistry which show a most intimate connexion existing between the cohesive state of a body and its chemical relations, and it would be quite superfluous to cite any such example.

Now, if by means of heat, and in some rare cases by that of light too, a number of simple substances can undergo an essential change as to their molecular aggregation, why should an agency so powerful as current-electricity, which parts asunder elements most intimately combined with one another;—I ask, why should such a force be incapable of modifying the natural cohesive state of bodies, for instance, that of iron? The conjecture, that the current which passes from that metal into a solution of blue vitriol modifies the molecular constitution of iron so as to destroy the affinity of the latter for the oxygen contained in the oxide of copper and disengaged from water by electrolytic action, is indeed a new and rather a bold one, but I should think no more so than many other conjectural views, which are now much in vogue with chemists and considered as very plausible. The fact that iron once being rendered inactive does not remain in its peculiar condition, is no proof against the correctness of my idea; for it may be said, that the modification of the molecular constitution caused by the current is, as it were, a condition forced upon the metal, a strained state, which ceases to exist as soon as its cause ceases to act. The condition may properly be compared to that in which the particles of a bent steel-spring exist.

The circumstance that inactive iron can remain untouched for any length of time within nitric acid of a certain strength without the agency of a current, is a fact which may appear to be quite irreconcilable with my hypothesis. I do not think, however, that such is the case. In the first place we must suppose that something like a "*vis inertiae*" comes into play, that is to say, to a certain degree a tendency of the iron particles to remain in their newly assumed juxtaposition; but on the other hand we must also admit the existence of some ac-

tion on the part of the acid upon the inactive iron. It is a well-known fact that fused phosphorus does not become solid at the common temperature if surrounded by a strong solution of potash*; and according to the experiments which I have made on the subject, phosphorus being in the circumstances mentioned can be cooled down nearly to the freezing point without becoming solid, whereas when covered with water it becomes solid at 104° Fahr. Now if the presence of a solution of potash prevent phosphorus from assuming its solid state, nitric acid by an analogous action may force the particles of inactive iron to remain in their peculiar relative position.

Having ventured myself so far into the regions of conjecture, I may, perhaps, be allowed to continue my course a little longer in that direction. For aught I know, all chemical philosophers tacitly acknowledge that the chemical attractive force which a particle of any element exerts with reference to a particle of any other simple body is equal on every one of its sides, provided the distance between the two particles remains the same. Now the peculiar state of iron leads me to suspect, that the particles of that metal have each of them two chemical poles, at least with regard to oxygen, one pole which attracts the latter body, and another pole which either exerts no attraction for oxygen or which repels it. Supposing each molecule of iron to be possessed of such polar sides, it may be conceived, that a current which passes from the metal into an oxi-electrolytic fluid, into a solution of blue vitriol for instance, directs these particles so as to place their attractive poles (attractive with regard to oxygen) towards the axis of the current, or inwards with respect to the surface of the metal, and the repulsive ones towards the electrolytic fluid. Such a position of the particles would prevent them from acting either upon the oxygen disengaged at them by electrolytic action or upon the oxygen contained in the oxide of copper. It is a matter of course, according to my hypothesis, that the arrangement of the poles of the molecules of the iron would be the reverse of that just spoken of, in case the metal acted the part of the cathode of a current. I have shown that a piece of iron rendered inactive by its having been made the positive electrode within common nitric acid, is turned active again by being made the negative one. I must, however, not omit to state, that in order to make the hypothesis agree with all the facts known respecting the peculiar condition of iron, we are obliged to suppose that the polarity of the iron particles exists only with regard to oxy-

* The presence of the solution of potash, however, is not required to retain the phosphorus in the liquid state. See *Phil. Mag. and Annals*, N.S., vol. iii. p. 144.—EDIT.

gen which is either in a combined state or set free by electrolytic action, and by no means to oxygen which is in its usual condition. If iron be voltaically associated with platina and put into water containing oxygen dissolved, the former metal is oxidized whether the platina be plunged into the fluid before or after the iron. If zinc be the substance put into voltaic association with iron, the latter is not in the least affected by the oxygen, which the water holds dissolved. For these last four months I have kept a combination of both metals within common water, which has been continually exposed to the air, and the surface of the iron is at this present moment as brilliant as it was when I put that metal into the water; whilst the zinc appears surrounded by a thick cloud of its oxide. In the two cases stated the electro-chemical laws hold good, whereas they do not at all agree with the phenomena which are referable to the peculiar condition of iron. I must openly confess that the different way in which the same current makes the iron act upon the oxygen appears to me as rather unfavourable to my hypothesis of a chemical polarity of the iron particles; but on the other hand it must be allowed, that the fact alluded to is likewise very much at variance with the principles of the electro-chemical system of the present day.

There is another objection to which my hypothesis will, perhaps, be thought liable. It may be said that the solid state of iron does not allow its particles the motion required for obtaining the peculiar arrangement of their poles mentioned. It is a point generally adopted by philosophers, that the atoms of no body do immediately touch each other, and it is supposed that the distance at which any two contiguous particles are placed from each other surpasses by far the diameter of each atom. If such be the case, I cannot conceive why the molecules of iron should not be capable of being turned by some force, being superior to that by which they are kept together under ordinary circumstances. There are, indeed, some facts which put it beyond doubt that the molecular constitution of a solid body may be essentially modified without having recourse to its liquefaction or vaporization. Gustave Rose, in a very interesting paper lately published in Poggendorff's *Annalen**, has demonstrated, that arragonite can easily be transformed into calcareous spar by moderately heating the former substance. Such a change cannot take place without an internal motion of the particles of carbonate of lime, the form of crystallization as well as the specific gravity of the compound becoming considerably modified under

* And of which a translation appeared in Lond. and Edinb. Phil. Mag., vol. xii. p. 465.—EDIT.

the circumstances. Now what heat is capable of effecting in one case, a current, I think, must be able to do in another.

As to the arrangement of the molecules of common iron with regard to the relative position of their chemical poles, it must be supposed to be similar to that of Ampère's molecular currents in the same metal before its being magnetized, that is to say, quite irregular. From such being the case it would follow that the surface of a piece of common iron is formed of attractive and repulsive poles, or what comes to the same thing, that the metal without being placed under the influence of a current of a certain direction, is to be chemically affected by oxygen which is in the peculiar state before mentioned.

It is not impossible that the supposed chemical polarity of the molecules of iron is in some way or other connected with the eminently magnetic properties of that metal, and it may even be imagined, that the current which is suspected of circulating round each iron particle has its source in the said polarity. As far as I know, Ampère has only postulated the currents in the iron, or rather inferred them from a certain number of facts, and given out no opinion whatever as to their ultimate cause. The passage of a current through iron must at any rate have some influence upon the relative position and motion of the supposed molecular currents of that metal, and changing the direction of these currents may also determine a modification of the chemical relations of iron. Pursuant to my hypothesis nickel and cobalt ought to be quite similar with regard to the phænomena of inactivity. Such, however, as formerly shown by me, is not the case; but on the other hand I must say, that my experiments were made upon such a small scale and were so few in number, that I do not yet dare to draw any conclusive inference from them.

Isomerism and dimorphism, generally speaking so closely connected with one another, are phænomena which have, perhaps, also something to do with chemical polarity. Up to this present moment they remain unaccounted for; but if we suppose that the particle of one substance exerts towards the particle of another different degrees of attraction, according to different relative positions of these molecules, we can conceive the possibility of two bodies forming a variety of distinct compounds, though the ratio in which their constituent parts enter into combination should remain the same.

Agreeably to such hypothesis a series of isomeric bodies would be nothing but [bodies constituted by the operation of] different sorts of chemical equilibrium between the same constituent parts. It would also follow from the hypothesis,

that amongst such a series of isomeric bodies, there is one in which the chemical equilibrium is stable, that is to say, the relative position of the chemical poles of the heterogeneous atoms such as to allow the greatest attraction between the component parts.

In closing this paper I cannot but repeat what I said at the beginning of it, namely, that I do not attach much importance to the views just now developed, they being entirely hypothetical; nevertheless I think them not altogether unworthy of being taken into consideration. If they should happen to excite happier ideas upon the subject treated of in my paper, they will not have been quite useless.

Bale, May 24, 1838.

XXXVI. *The Specific Heats of the Gases as deduced by Dr. Apjohn, compared with the more recent Results of Dr. Suerman.* By JAMES APJOHN, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Ireland.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT is known to some of my scientific friends that I have been for a considerable time engaged in experiments for determining the specific heats of the more remarkable gaseous bodies; and indeed several of the results at which I have arrived have been communicated to the public through the reports of the British Association and the pages of the *Philosophical Magazine*.* The entire of my researches on this subject have some time since been laid before the Royal Irish Academy, and have appeared in a connected form in the volume of its Transactions which has just issued from the press. As, however, the circulation of the Transactions is necessarily limited, and, also, in order that I may set myself right with the public upon some points in reference to which I have been misapprehended, I am anxious to avail myself of the pages of your Journal, to draw attention to the efforts which I have made towards the solution of a problem of acknowledged difficulty and great importance. I have also another object in making this hasty communication, viz. to bring under the notice of British philosophers the recent very able and valuable investigations of Suerman, a copy of whose memoir on the specific heats of gases has recently come into my possession.

Dr. Suerman has, as will be seen, adopted my method, and, by means of a very elaborate apparatus, has arrived at conclusions which, as I shall show, correspond very closely with

[* See L. and E. Phil. Mag., vol. vii. p. 385.]

mine. Let me not, however, be misunderstood. Dr. Suerman has borrowed nothing from me, for before he had seen my first paper on specific heats he had resolved upon employing the method in question in the same research*. So far from having reclamations to make, I feel myself his debtor. He has frankly admitted my priority, and spoken of my experiments in terms I fear much too flattering.

Before proceeding to my immediate object it will be necessary to remind the reader that in November 1834†, I communicated the following formula for the solution of the dew-point problem to the Royal Irish Academy :

$$f'' = f' - \frac{d}{88} \times \frac{p}{30}.$$

in which f'' is the force of vapour at the dew-point, f' the same at the temperature of the wet thermometer, $d = t - t'$ the difference between the indications of the wet and dry instruments, p the existing, and 30 the mean pressure.

In investigating this expression it was assumed that the specific heat of air and the caloric of elasticity of aqueous vapour are constant, at least within the limits of the variations, in these latitudes, of atmospherical temperature and pressure, an hypothesis the strict accuracy of which cannot be admitted. Preparatory therefore to the application of the formula to the investigation of the specific heats of gases, it became necessary to give it its most comprehensive form, substituting for the

numeral coefficient $\frac{1}{88}$ the factors of which it is composed, and introducing the consideration of density, in order that the expression may be true generally of the various elastic fluids. The steps which have conducted to such general expression I shall here give, partly because I have not published them elsewhere, and partly because my investigation of the

formula $f'' = f' - \frac{d}{88} \times \frac{p}{30}$ has been by some considered as complicated and obscure. When, in the case of the wet thermometer, the stationary temperature is attained, the caloric which vaporizes the water is necessarily exactly equal to that which the surrounding gas imparts in descending from its proper temperature to that of the moistened bulb. From

* "Tandem opus aggressus, et occupatus in idoneo parando suppellectili, diarium accepi Anglicum, quo in collegio, quod Dublini habetur, Chemiæ professoris Apjohn continebatur disquisitio, ex eodem illo principio fluidorum elasticorum calorem specificum derivans. Primum, quid sileam? Animo despondebam, quum novitatis colorem, quæ mihi præcipue ardebat, de meo evanescere viderem proposito." (Preface, p. viii.)

† See Lond. and Edinb. Phil. Mag. vol. vi. p. 182.]

this consideration, and the additional hypothesis that the gas so cooled by successive contacts with the moistened bulb is saturated with humidity, we can deduce the following equation:

$$f'' = f' \left(1 - \frac{m'}{m} \right)$$

in which f'' or f' have the significations already assigned to them, while m' and m represent, the former the amount of vapour which would be formed by the caloric evolved from a given bulk of the gas in cooling through $t-t'$ degrees, the latter the maximum quantity which the same volume of such gas could contain at the temperature t' . The correctness of this deduction is easily shown. For m being the total quantity of moisture in the gas, and m' the quantity introduced into it, $m-m'$ will be the quantity it already contained; so that, since, when the temperature and volume of vapour are both given, its elastic force is proportional to its quantity, we shall have

$$m : m-m' :: f' : f''$$

a proportion which gives, as above,

$$f'' = f' \left(1 - \frac{m'}{m} \right).$$

In arriving at this conclusion, we have assumed that the air which is cooled by contact with the moistened bulb becomes saturated with humidity. This is the only premiss which we have employed about which a question can be raised. That it is brought, however, into such condition, no one can, I conceive, entertain a doubt who reflects upon the very low conducting power of air, and the consequent impossibility of its communicating caloric to the moistened bulb unless by actual contact, a condition which can scarcely be fulfilled without the entire of the cooled air being at the same time carried to the maximum degree of humidity.

In the expression for f'' given above, f' may be considered as known, the corresponding temperature t' being the result of an observation. In order, therefore, to render the formula available it will only be necessary to determine in known terms values of m' and m , which may be done in the following manner:

Let a be the specific heat of the gas under a constant pressure, and e the caloric of elasticity of aqueous vapour at t' , the stationary temperature of the wet thermometer. It is evident that a grain of the gas in cooling through $t-t' = d$ degrees gives out the caloric necessary for raising the temperature of a grains of water through the same number of degrees. But the caloric which heats a grains of water d degrees would

convert into vapour having the temperature t' an amount of the same fluid represented by $\frac{ad}{e}$ grains. Hence one grain of the gas in cooling through d degrees evolves heat adequate to the evaporation of $\frac{ad}{e}$ grains of water at the temperature shown by the wet thermometer; or, if we divide by $\frac{a}{e}$, $\frac{e}{a}$ grains of the gas in cooling through d degrees extricates heat which will vaporize d grains of moisture at t' . For m' therefore in the formula f

$$f'' = f' \left(1 - \frac{m'}{m}\right),$$

d may be substituted.

Now, supposing the gas to be atmospheric air, and that 100 cubic inches of this weigh 31 grs., the volume in cubic inches of $\frac{e}{a}$ grains of it at 60° and under a pressure of 30 will obviously be

$$\frac{e}{a} \times \frac{100}{31}$$

and at any other temperature t' , and pressure p ,

$$\frac{e}{a} \times \frac{100}{31} \times \frac{448+t'}{508} \times \frac{30}{p},$$

an expression which, as the volumes are reciprocally as the densities, will, for any gas whose specific gravity is s , become

$$\frac{e}{as} \times \frac{100}{31} \times \frac{448+t'}{508} \times \frac{30}{p}.$$

Let this be multiplied by $\frac{508}{448+t'} \times \frac{f'}{30} \times \cdot 625 \times 31$, the weight of a cubic inch of aqueous vapour of maximum tension at temperature t' , and the product, viz.

$$\frac{e}{as} \times \frac{100}{31} \times \frac{448+t'}{508} \times \frac{30}{p} \times \frac{508}{448+t'} \times \frac{f'}{30} \times \cdot 625 \times 31 = \frac{e}{as} \times \frac{f'}{p} \times \cdot 625,$$

will be the maximum amount of moisture which can be contained in $\frac{e}{a}$ grains of the gas at temperature t' , and pressure p , and is therefore the value of m .

If therefore we revert to the equation

$$f'' = f' \left(1 - \frac{m'}{m}\right),$$

and substitute in it for m' and m their values, namely d , and $\frac{e}{as} \times \frac{f'}{30} \times .625$, we are conducted to the following final equation, which includes the solution of the dew-point problem,

$$f'' = f' - \frac{sa d}{e} \times \frac{p}{.625} = f' - \frac{48 sa d}{e} \times \frac{p}{30} \quad (\text{A.})$$

In the case of atmospheric air $s = 1$; so that for the purposes of the meteorologist

$$f'' = f' - \frac{48 a d}{e} \times \frac{p}{30}, \quad (\text{B.})$$

which, if we assume $a = .267$, and $e = 967^\circ + 212^\circ - 50^\circ = 1129$, becomes

$$f'' = f' - \frac{d}{88} \times \frac{p}{30},$$

the expression which I have used in my paper on the dew-point. Now though a , and e are not, as is assumed in this latter expression, constant, the mean values which are assigned to them are sufficiently exact for all practical purposes. This I believe I may say I have established by three distinct series of experiments, for the particulars of which, however, I must refer to my second paper on the dew-point.

Before proceeding to other topics, I wish to state that an expression identical with (B) was obtained some years ago by Mr. Ivory. His method, however, was so totally different from mine, that those who examine both will, I make little doubt, consider it scarcely necessary for me to disclaim, as I have elsewhere done, any knowledge of his investigations at the time I gave publicity to my own results. In perusing also Dr. Suerman's thesis already referred to, I find ascribed to Gay-Lussac a formula for *dry* air which scarcely differs, as shall be hereafter shown, from one which may be deduced from mine. Again, in the first part of Professor Graham's Elements of Chemistry recently published, and which he was good enough to send me, I find, in a note to page 82, a formula ascribed to Dr. August which is in form the same with that which I have given, but adapted to Reaumur's thermometer. The coefficient, however, of d is too great by about one fourth; for the formula in question (adopting in it the notation already employed,) is

$$f'' = f' - \frac{.558 d p}{e},$$

whereas, assigning to a its ordinary value .267, my expression, when brought to this shape, would be

$$f'' = f' - \frac{.4272 d p}{e}.$$

Now, the only way of accounting for this difference is by supposing Dr. August to consider the value of a , the specific heat of air, to be not .267 but .349, an assumption which it is scarcely necessary to say is altogether inconsistent with experiment. Professor Graham observes of this formula, "It was employed by Humboldt and G. Rose in their recent expedition to Siberia, and (as I was assured by the latter) with excellent effect." This testimony would have startled me much had it been stated that contemporaneous observations were uniformly made with a condensation hygrometer. But as this is not asserted, I feel the less reluctance in declaring that the formula in question, however satisfactory it may seem to the eminent philosophers just named, is undoubtedly erroneous. In an observation of theirs quoted by Professor Graham, t being $74^{\circ}9$, t' $56^{\circ}9$, and p 30.17 , by the method of August, f'' is inferred to be .22678, whereas by mine it would be .26596. Adopting the former value the dew-point t'' would be $35^{\circ}6$, while, if the latter be correct, it is $40^{\circ}1$. Now from the many and severe tests to which I have put my own method, I do not hesitate to assert that the first-mentioned determination of the point of deposition makes it at least four degrees Fahrenheit too low.

But to return from this digression to the subject of specific heats. From equation (A) or $f'' = f' - \frac{48 s a d}{e} \times \frac{p}{30}$ we readily deduce the following expression for the specific heat of a gas, viz.

$$a = \frac{(f' - f'')e}{48 s d} \times \frac{30}{p},$$

which, when the gas is perfectly dry, or, in other words, where $f'' = 0$ becomes

$$a = \frac{f' e}{48 s d} \times \frac{30}{p}.$$

This is the specific heat of the gas under a given weight. Hence as the specific heats of equal volumes of different gases are equal to the specific heats of equal weights multiplied by the

specific gravities, the general expression for the specific heats of equal volumes will be

$$\frac{f'e}{48d} \times \frac{30}{p} \quad (C.)$$

Now, in this expression $d = t - t'$, f' the force of vapour at t' , and p the existing pressure, are given by observation; e is also a known quantity, being, according to the most accurate experiments, and upon the hypothesis that the sum of the sensible and latent heat of aqueous vapour at all temperatures is a constant quantity, equal to $1179 - t$. By aid, therefore, of a hygrometric observation with a wet and dry thermometer in any gas, the barometer being also observed, the specific heat of a given volume of such gas may be calculated. Such is the method which, as far as my knowledge extends, I have been the first to propose and practise. To this general explanation it is only necessary to add that, at the close of each experiment the gas operated with was subjected to analysis, and a correction made for the per centage of atmospheric air which it was found to include, by means of a formula which may be thus investigated.

Let a be the calculated specific heat of the mixture of air and gas, a' the true specific heat of the gas, n the number of volumes of air in 100 of the gas, and c the specific heat of air as deduced by means of formula (c) from a distinct experiment with air alone. Then, on the principle that the specific heat of the mixture of air and gas multiplied by its volume is equal to the sum of the products of the respective volumes of air and gas multiplied by their respective specific heats,

$$a' (100 - n) + n c = 100 a$$

which gives

$$a' = a + \frac{(a - c)n}{100 - n}.$$

As examples of the course to be pursued, I shall adduce the following observations made successively on air and hydrogen, on the 5th of August 1835.

| | t | t' | d | p |
|----------|-----|------|-----|--------|
| Hydrogen | 68 | 48 | 20 | 30.114 |
| Air..... | 68 | 43 | 25 | 30.114 |

By applying to these results the equation $a = \frac{ef'}{48d} \times \frac{30}{p}$

we get

Specific heat of air = .2767 = c

Specific heat of hydrogen = .4090 = a .

But the hydrogen upon analysis was found to contain 5 per

cent. of air. Hence, the specific heat of the hydrogen supposed pure, as deduced from the equation $a' = a + \frac{(a-c)n}{100-n}$,

becomes .4151. And as .2767 : .2670 :: .4151 : .4005, the specific heat of hydrogen compared to that of an equal volume of air under a pressure of 30 when water is represented by unity, or, what amounts to the same, when air is .267.

In order to the determination of t and t' the wet and dry thermometers were introduced into a glass tube through which the gas, first dried by bubbling through oil of vitriol, was made to pass in a rapid current, by means of pressure with a board upon a large bladder in which it was contained; and, as soon as the wet thermometer, which by exposure to the dry air or gas rapidly falls, acquired a stationary temperature, its indication and that of the dry instrument were registered. In the case of air there was no difficulty in reaching this stationary point, it being obviously only necessary to maintain the blast sufficiently long. It would, however, have been inconvenient to employ a sufficient quantity of the other gases to ensure the production in each case of a maximum degree of depression; so that, when for any of these t and t' had to be observed, the wet thermometer was first brought nearly to its stationary point by a current of atmospherical air. The depression proper to the gas was now easily obtained by causing the blast of air to be *immediately* succeeded by one of the gas, and which in consequence of this contrivance it was, generally speaking, found necessary to maintain only for a few seconds. For a more detailed description, and a wood-cut of the apparatus, I must refer to the report published by the British Association of its proceedings in Bristol. The results obtained by the method just explained are presented in the following tables. Table (1.) relates to atmospherical air alone; table (2.) to the other gases. In table (3.) we have the mean results given in table (2.) referred to atmospherical air represented both by .267 and by unity. The numbers in the last column of table (3.) are the specific heats of equal weights, and are got by dividing the numbers in the preceding column by the specific gravities of the gases to which they respectively belong.

(1.)

| | <i>t</i> | <i>t'</i> | <i>d</i> | <i>p</i> | <i>a</i> |
|----------|----------|-----------|----------|----------|----------|
| June 21. | 58·8 | 38·4 | 20·4 | 30·014 | ·2912 |
| 27. | 52·7 | 34·9 | 17·8 | 30·225 | ·2935 |
| July 31. | 64·5 | 41·2 | 23·3 | 30·330 | ·2773 |
| Aug. 1. | 67·3 | 42 | 25·3 | 30·140 | ·2624 |
| 4. | 68 | 43 | 25 | 30·114 | ·2767 |
| 5. | 67 | 42·4 | 24·6 | 30·000 | ·2768 |
| 7. | 66 | 44·7 | 24·3 | 30·218 | ·2657 |
| | | | | | ·2776 |

(2.)

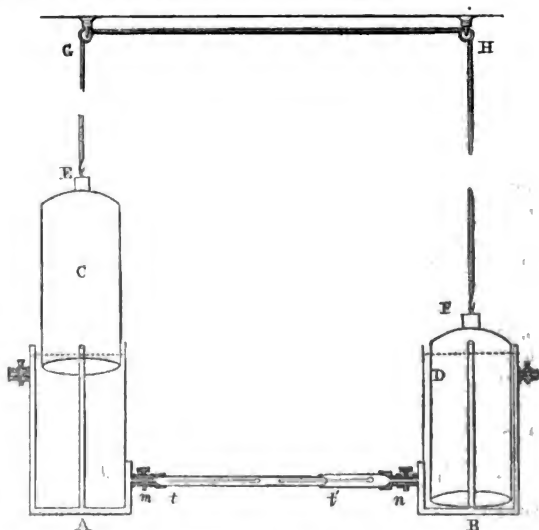
| | <i>t</i> | <i>t'</i> | <i>d</i> | <i>p</i> | <i>a</i> | Air per cent. | <i>a'</i> | <i>a''</i> | Means. |
|--------------------|----------|-----------|----------|----------|----------|---------------|-----------|------------|--------|
| Azote..... { | June 27. | 53·8 | 35·5 | 18·3 | 30·225 | ·2915 | | ·2915 | ·2912 |
| | July 31. | 65 | 41·3 | 23·7 | 30·330 | ·2735 | | ·2735 | ·2669 |
| | | | | | | | | | ·2660 |
| Carbonic Acid... { | June 21. | 60· | 40 | 20 | 30·014 | ·3135 | 11·4 | ·3137 | ·2876 |
| | 27. | 53·8 | 36·5 | 17·3 | 30·225 | ·3178 | 12 | ·3211 | ·2921 |
| | July 31. | 65·2 | 42·7 | 22·5 | 30·330 | ·3021 | 8·2 | ·3043 | ·2933 |
| | | | | | | | | | ·2910 |
| Carbonic Oxide. { | Aug. 1. | 67·3 | 43·5 | 23·8 | 30·140 | ·2952 | | ·2952 | ·3003 |
| | 5. | 67·5 | 43 | 24·5 | 30·000 | ·2874 | | ·2874 | ·2772 |
| | 7 | 66·2 | 42·4 | 23·8 | 30·218 | ·2774 | | ·2774 | ·2825 |
| | | | | | | | | | ·2863 |
| Hydrogen { | June 21. | 59 | 42·8 | 16·2 | 30·014 | ·4262 | 4 | ·4317 | ·3961 |
| | 27. | 52·3 | 38·9 | 13·4 | 30·225 | ·4475 | 7 | ·4590 | ·4175 |
| | July 31. | 65 | 46 | 19 | 30·330 | ·4000 | 7·5 | ·4099 | ·3946 |
| | Aug. 4. | 68 | 48 | 20 | 30·114 | ·4092 | 5 | ·4151 | ·4005 |
| | | | | | | | | | ·4022 |
| Nitrous Oxide { | Aug. 4. | 67·5 | 44·5 | 23 | 30·114 | ·3173 | 27·5 | ·3327 | ·3210 |
| | 7. | 65 | 42·5 | 22·5 | 30·218 | ·3013 | 14 | ·3071 | ·3085 |
| | | | | | | | | | ·3147 |

(3.)

| | Specific Heats of equal Volumes. | | Specific Heats of equal Weights. |
|-------------------------------|----------------------------------|-------|----------------------------------|
| Atmospheric Air | ·2670 | 1·000 | 1·000 |
| Azote..... | ·2660 | ·996 | 1·024 |
| Oxygen, (by calculation)..... | ·2710 | 1·015 | ·920 |
| Hydrogen | ·4022 | 1·506 | 21·826 |
| Carbonic Acid | ·2910 | 1·090 | ·715 |
| Carbonic Oxide | ·2863 | 1·072 | 1·102 |
| Nitrous Oxide | ·3147 | 1·179 | ·773 |

Upon these results I never placed much reliance. The apparatus employed was very imperfect, particularly in not allowing more than a single experiment with the same quantity of gas; and I also saw reason to doubt that I had in every instance by means of it accomplished perfect desiccation. The difference, however, between my number for hydrogen and that of De la Roche and Berard, which has hitherto

been generally adopted, appeared to me much too great to admit of being referred to error of experiment. I was therefore very anxious to return to the subject, and towards the end of July 1836, I undertook a fresh series of experiments, which were conducted on the following plan.



A pair of copper gasometers, A, B, with glass bells, C, D, such as are usually employed by chemical lecturers, were charged with a proper quantity of oil of vitriol, instead of water, and placed upon a table at the distance of three feet from each other, the brass caps, E, F, attached to the bells, being suspended to the extremities of a stout cord passing over a pair of pulleys, G, H, fixed in the ceiling of the laboratory, the length of the cord being such that while one of the bells was almost entirely immersed in the oil of vitriol, the other dipped about an inch beneath its surface. Between the lower stopcocks, *m*, *n*, attached to the gasometers, a couple of glass tubes were interposed, connected to the stopcocks by caoutchouc collars, and fitting at their other extremities to each other by a tight ground joint. In the larger of these tubes the dry thermometer *t* was permanently placed, and into it the wet one *t'* was also introduced previous to the commencement of an experiment. Matters being, we shall suppose thus prepared, and the unimmersed bell, C, occupied,

first, with atmospherical air deprived by the oil of vitriol of its moisture, pressure was made upon it by an assistant, so as to force its contents in a rapid current into the second bell, D, through the tubes containing the wet and dry thermometers. During this operation the observer kept his eye, armed with a lens, steadily fixed on the thermometers, and registered the indications of both as soon as the wet one became and continued stationary for a few seconds. The height of the barometer being now taken, the necessary *data* were obtained for calculating from formula (A), or

$$f'' = f' - \frac{48 a d}{e} \times \frac{p}{30},$$

the elastic force of the vapour still existing in the air of the gasometer. This air being now replaced by one of the gases which were to be the subject of experiment, and this being left during the same time with the air in contact with the oil of vitriol, the very manipulations and observations just detailed were repeated. This same experiment was again and again performed, and it having been ascertained, after a considerable number of repetitions, that the results were uniform and consistent, and that they might therefore be relied upon, the mean of all the observations was taken, and from this the specific heat of the gas was deduced by means of the formula

$$a = (f' - f'') \frac{e}{48 d} \times \frac{p}{30},$$

that value being assigned to f'' which resulted from formula (A) applied to the preliminary experiments on atmospherical air. The analysis of the gas was next very carefully performed, and it having been ascertained that n volumes, e. g. of atmospherical air per cent. were present, the proper correction was applied by the formula

$$a' = a + \frac{(a - e)n}{100 - n}$$

already explained, in which $e = .267$ is the specific heat of air, a' the true specific heat of the gas, and a the specific heat of mixture of air and gas as previously determined. Such was the course pursued in the case of each of the gases submitted to experiment.

The particulars of the entire series of experiments are comprehended in tables (1.) and (2.) Table (3.) contains the final results, alongside which are placed the numbers of De la Roche and Berard, and those of Dulong, for the purpose of comparison.

(1.)

| 1836. | <i>t</i> | <i>t'</i> | <i>p</i> | <i>d</i> | <i>f''</i> |
|----------|----------|-----------|----------|----------|------------|
| Aug. 8. | 63·5 | 40·3 | 23·2 | 30·226 | ·0024 |
| 8. | 63·2 | 40·1 | 23·1 | 30·226 | |
| 8. | 63·2 | 40 | 23·2 | 30·226 | |
| Mean ... | 63·3 | 40·1 | 23·2 | 30·226 | |
| Aug. 9. | 62·8 | 40·5 | 22·3 | 30·250 | |
| 9. | 63 | 40·8 | 22·2 | 30·250 | ·0134 |
| 9. | 62·9 | 40·6 | 22·3 | 30·250 | |
| 9. | 63·5 | 41·1 | 22·4 | 30·250 | |
| 9. | 63·2 | 40·8 | 22·4 | 30·250 | |
| Mean ... | 63·1 | 40·7 | 22·4 | 30·250 | |
| Aug. 10. | 63 | 40·7 | 22·3 | 30·208 | ·0114 |
| 10. | 64 | 41·5 | 22·5 | 30·208 | |
| 10. | 63·4 | 41·2 | 22·2 | 30·208 | |
| 10. | 63·6 | 40·9 | 22·7 | 30·208 | |
| Mean ... | 63·5 | 41·1 | 22·4 | 30·208 | |
| Aug. 11. | 62·2 | 40·9 | 21·3 | 30·310 | ·0241 |
| 11. | 63 | 40·9 | 22·1 | 30·310 | |
| 11. | 63 | 41·5 | 21·5 | 30·310 | |
| 11. | 63·8 | 41·5 | 22·3 | 30·310 | |
| 11. | 63·2 | 41·7 | 21·5 | 30·310 | |
| 11. | 63·2 | 41·3 | 21·9 | 30·306 | ·0063 |
| 11. | 64 | 41·4 | 22·6 | 30·306 | |
| Mean ... | 63·2 | 41·3 | 21·9 | 30·307 | |
| Aug. 12. | 66·5 | 41·8 | 24·7 | 30·270 | |
| 15. | 65·8 | 41·9 | 23·9 | 30·070 | |
| 15. | 66·5 | 41·4 | 25·1 | 30·070 | ·0027 |
| 15. | 66·6 | 41·9 | 24·7 | 30·070 | |
| Mean ... | 66·3 | 41·7 | 24·6 | 30·070 | |

(2.)

| 1836. | <i>t</i> | <i>t'</i> | <i>d</i> | <i>p</i> | <i>α</i> | Air
per Cent. | <i>g'</i> |
|---------------|----------|-----------|----------|----------|----------|------------------|-----------|
| Carbonic Acid | Aug. 8. | 62·5 | 41·5 | 21 | 30·226 | 10·2 | ·3992 |
| | 8. | 62·6 | 41·5 | 21·1 | 30·226 | | |
| | 8. | 63 | 41·9 | 21·1 | 30·226 | | |
| | 8. | 63 | 42·3 | 20·7 | 30·226 | | |
| | 8. | 63·6 | 42·4 | 21·2 | 30·226 | | |
| | 8. | 63·7 | 42·4 | 21·3 | 30·226 | | |
| Hydrogen..... | 8. | 63·2 | 42·1 | 21·1 | 30·226 | 11·4 | ·4012 |
| | Mean ... | 63·1 | 42 | 21·1 | 30·226 | 10·8 | |
| | Aug. 8. | 63·6 | 45·1 | 18·5 | 30·226 | 3·2 | |
| | Aug. 9. | 62·1 | 44·1 | 18 | 30·260 | 3·6 | |
| | 9. | 62·6 | 44·1 | 18·5 | 30·260 | 4 | |
| | 9. | 63 | 44·7 | 18·3 | 30·260 | 4·4 | |
| Hydrogen..... | 9. | 62·8 | 44·7 | 18·1 | 30·260 | 4·8 | ·3781 |
| | 9. | 63 | 44·6 | 18·4 | 30·260 | 5·2 | |
| | 9. | 63·8 | 45·1 | 18·7 | 30·250 | 5·7 | |
| | 9. | 63 | 44·6 | 18·4 | 30·250 | 4·3 | |
| | Mean... | 62·9 | 44·5 | 18·4 | 30·257 | 14 | |
| | Aug. 10. | 62·1 | 42·1 | 20 | 30·200 | 16 | ·3186 |
| Nitrous Oxide | 10. | 63 | 42·4 | 20·6 | 30·205 | | |
| | 10. | 63·6 | 42·6 | 21 | 30·210 | | |
| | 10. | 63 | 42 | 21 | 30·210 | | |
| | 10. | 63·5 | 42·5 | 21 | 30·210 | | |
| | 10. | 63·4 | 42·6 | 20·8 | 30·210 | | |
| | Mean... | 63·1 | 42·3 | 20·8 | 30·207 | 3109 | |

TABLE (2) *continued.*

| 1836. | | <i>t</i> | <i>t'</i> | <i>d</i> | <i>p</i> | <i>a</i> | <i>a'</i> |
|---|----------|----------|-----------|----------|----------|----------|-----------|
| Equal volumes
of Carb. Acid
and Carb. Oxide | Aug. 11. | 64.9 | 42.8 | 21.1 | 30.306 | | |
| | 11. | 65.4 | 43.3 | 22.1 | 30.306 | | |
| | 11. | 65.3 | 43.3 | 22 | 30.306 | | |
| | Mean... | 65.2 | 43.1 | 22.1 | 30.306 | .2865 | .2865 |
| Equal volumes
of Carb. Acid
and Carb. Oxide | Aug. 12. | 65.3 | 42.7 | 22.6 | 30.27 | | |
| | 12. | 65.8 | 43.4 | 22.4 | 30.27 | | |
| | 12. | 65.8 | 42.3 | 23.5 | 30.27 | | |
| | 12. | 65.3 | 42.6 | 22.7 | 30.27 | | |
| | Mean... | 65.5 | 42.7 | 22.8 | 30.27 | .2988 | .2988 |
| Nitrogen | Aug. 15. | 64.8 | 41.9 | 22.9 | 30.07 | | |
| | | 66.8 | 42.3 | 24.5 | 30.07 | | |
| | | 66 | 42 | 24 | 30.07 | | |
| | Mean... | 65.9 | 42.1 | 23.8 | 30.07 | .2799 | .2799 |

(3.)

Specific Heats of equal Volumes.

| | J. A. | | De la Roche
and
Berard. | Dulong. |
|---------------------|-------|-------|-------------------------------|---------|
| Atmospheric Air ... | .2670 | 1.000 | 1.000 | 1.000 |
| Nitrogen | .2799 | 1.048 | 1.006 | 1.000 |
| Oxygen | .2154 | .808 | .976 | 1.000 |
| Hydrogen | .3896 | 1.459 | .900 | 1.300 |
| Carbonic Acid ... | .3192 | 1.195 | 1.258 | 1.172 |
| Carbonic Oxide ... | .2660 | .996 | 1.034 | 1.000 |
| Nitrous Oxide | .3186 | 1.193 | 1.350 | 1.159 |

The number in this latter table for oxygen was inferred from that for nitrogen by formula (C), and the same may be said of the number attached to carbonic oxide, which was calculated in the same way from the specific heat of carbonic acid and of a mixture of equal volumes of the two gases, as deduced from experiment. From the care bestowed upon the various manipulations, and the consistency of the different observations in the same gas, I am disposed to look upon the numbers given above as very close approximations to the truth. I should probably except from this statement the specific heats ascribed to oxygen and nitrogen, as but three experiments were made, in consequence of one of the gasometers having begun to leak. Moreover as nitrogen was the gas operated with, in passing by calculation to the specific heat of oxygen the errors of observation would be multiplied by four. Oxygen, in fact, not nitrogen, should have been the subject of experiment.

[To be continued.]

XXXVII, *A Remark on an Article of M. Poisson's Traité de Mécanique (Edition 2nd. No. 593.).* By JAMES IVORY, K.H., F.R.S., &c.*

IN speculations of difficulty it is of great importance to note such points as are susceptible of clear demonstration. What is thus established by undoubted evidence, is not liable to be misapprehended or inadvertently misapplied. In this view it may be useful to demonstrate the following theorem, relating to the equilibrium of incompressible fluids, the particles of which are urged by accelerating forces: *If an interior level surface be extended through the mass, the body of fluid within the level surface will be in equilibrium independently of the rest of the mass, and supposing the incumbent fluid were removed.*

In order to demonstrate this theorem, suppose a canal to be conducted from an orifice in the upper surface of the fluid to the central point within all the level surfaces: the pressure of this canal at the centre, caused by all the forces which urge its elementary portions in the direction of the canal, and estimated on the unit of surface, will be the same, whatever be the position of the initial point in the upper surface: the symbol B may be used to denote the intensity of the pressure at the centre resulting from this canal, which is no other than the central column of Newton. In like manner if a similar canal be drawn to the centre from any orifice in an interior level surface, the intensity of pressure at the centre, represented by b , will be a constant quantity. The intensity of the exterior pressure at all the points of the level surface, caused by all the forces that urge the particles of the incumbent fluid, will be equal to $B - b$. Using the same symbols as M. Poisson (Edition 2nd, No. 583), x, y, z will represent the three rectangular coordinates of a particle of the fluid; X, Y, Z , the accelerating forces acting on the particle in the directions of x, y, z ; and, as unit may stand for the density of an incompressible fluid, we shall have

$$B - b = \int (X dx + Y dy + Z dz) \dots\dots\dots (a.)$$

the integral representing the intensity with which any canal, having one orifice in the upper surface of the fluid and the other in the level surface, presses upon the level surface. By differentiating the equation, supposing the coordinates to vary in the level surface, we obtain

$$X dx + Y dy + Z dz = 0 \dots\dots\dots (b.)$$

* Communicated by the Author.

which expresses that the forces X, Y, Z are ineffective to displace the particles in any direction upon the level surface, their resultant being perpendicular to that surface. The signification of these equations being explicitly settled, it readily follows that they are alone sufficient to determine the figure of equilibrium, when there are no other causes tending to displace a particle in a level surface, except the pressure of the incumbent fluid caused by all the forces, of whatever description, that urge its particles.

When the fluid consists of particles that attract one another, M. Poisson admits that the matter exterior to a level surface will attract the particles placed in that surface, or within it. The effect of this attractive force is perfectly distinct from the pressure of any canal passing between the upper surface and the level surface. In neither of the equations (a) nor (b) is any account taken of an attraction which the exterior fluid exerts upon a particle in a level surface. But if the resultant of such attractions be not perpendicular to the level surface at every point, the particles in that surface will be displaced, and there will be no equilibrium. Let P, Q, R , represent the partial attractions of the matter exterior to a level surface, upon a particle in that surface, the forces being respectively parallel to x, y, z , the coordinates of the particle: then the condition that the attractive forces shall be ineffective to move the particle in any direction upon the level surface; or, which is the same thing, the condition that the resultant of the attractions shall be perpendicular to that surface, is thus expressed,

$$\left. \begin{aligned} P dx + Q dy + R dz &= 0 \\ \int (P dx + Q dy + R dz) &= \text{const.} \end{aligned} \right\} \quad (c.)$$

the coordinates varying in the level surface. Now these equations are not less indispensable than the former ones (a) and (b), to the immobility of the particles of a level surface. And thus it appears that, when all the forces in action are taken into account, two independent conditions are necessary for determining the figure of equilibrium of a fluid at liberty, which consists of attracting particles; a conclusion that accords perfectly with the result deduced from analytical considerations in this Journal for August last (p. 81).

What has been proved is inconsistent with the argument of M. Poisson in the article (No. 593) cited from his *Mécanique*. For, as it is here shown that the whole action of the exterior matter, whether by attraction or by pressure, upon the particles in a level surface, is directed perpendicularly to that surface, it follows that the removal of the exterior matter

will not alter the state of the fluid within the level surface with respect to an equilibrium. On the contrary, M. Poisson alleges that a necessary consequence of removing the exterior matter would be a change in the figure of the fluid body within the level surface.

The discrepancy is easily explained. M. Poisson makes the equilibrium depend entirely on the equations (*a*) and (*b*); that is, he considers only the pressure of the exterior matter caused by the forces which urge its own particles, neglecting the attractive force of the same matter upon the interior fluid. When the effect of this attraction was pointed out, he gave the explanation of it in his work referred to. What he should have done was, to return upon his steps, and to correct his investigation by taking into account all the forces tending to move the particles of the fluid.

A general demonstration of the theorem will readily suggest itself from what has been said. To elucidate the principle of *the equality of pressure in all directions*, and to point out what may safely be inferred from it, would exceed the present limits.

Sept. 12, 1838.

J. IVORY.

XXXVIII. *On some of the Phenomena and Laws of Action of Voltaic Electricity, and on the Construction of Voltaic Batteries, &c.* By CHRISTOPHER BINKS. *A second Communication, addressed to J. F. Daniell, Esq. F.R.S., &c., Professor of Chemistry in King's College, London. Part the First.*

[Continued from p. 180.]

159. **A**N examination of these results as they are shown by these tables, and by the diagram, would seem to warrant the following general inferences.

160. 1st inference. That considering the zinc in these arrangements as the generating point, and the mass of liquid intermediate between it and the copper to be placed in an electrical condition, in whatever such a condition may consist, whether in the actual transmission through the liquid of a material agent, or the disposing of its particles into a state of polarization, or of induction, or in what; considering the electricity developed at the zinc to exercise an influence of this nature over the intermediate mass, we must infer from these experiments, that that influence is diffused over a greater space as we progressively recede from the point from which it emanates, whilst its quantity, howsoever distributed, remains precisely the same. So that at the several distances of

1, 2, and 4 inches above, we obtain this influence in the same amount, but find it to be distributed over spaces progressively increasing with the distance.

161. Or the inference to be drawn from these facts may be otherwise expressed ; that the amount of electrical action occurring in such arrangements as those here employed, may be condensed within a smaller space, or diffused over a larger one, under certain limitations in both cases, in proportion as the elementary conducting plate is approached to, or removed from, the zinc or generating point, presenting in these operations a marked analogy to the general phænomena of the radiation of light and heat.

162. But in this kind of reasoning there are two assumptions ; first, that the zinc is the point from which this influence emanates ; and second, that the mass of liquid interposed between the two plates is placed in any peculiar condition ; and for neither of these have we the support of any direct evidence.

163. Light and heat radiate in all directions from any generating point, as from one common centre. It matters not to the present purpose by what theory we explain the mode of emanation, or whether we assume their materiality or the contrary. We know of their presence at any given distance from the point of emanation, only by the interposition (as in the case of light), of any body capable of receiving its impressions, or of reflecting it ; and in the case of heat, in like manner, only by the agency of some body interposed, and capable of appreciating or indicating its existence within the medium through which it is supposed to be diffused or propelled. The same agencies which serve to detect the presence of light and heat, serve also to measure their quantities. And in like manner does the copper-plate, in such voltaic arrangements as the above, serve to indicate the existence of voltaic action, and to measure its amount. The copper-plate may then be considered as subserving the same purpose in electrical experiments, as the screen in optical experiments, and the thermometer in those of heat ; but with this (though perhaps only apparent) difference, that the copper-plate is essential equally to the production as to the measurement of electricity ; whereas in the other two, their production is apparently independent of the instruments by which their quantities are determined. But making no attempt to trace the existence of any analogy as regards the way in which these several agents are originally produced, there seems, so far, to be some evidence for believing, that when once produced, some of the phænomena they exhibit are similar in general character ; and as far as regards

one such at least, namely, that of radiation, as far as that is developed in these experiments, the analogy between them appears to be complete.

164. Reasoning upon the facts derived from experiment and by analogy, it thus appears that there is evidence for believing that this divergence extends within the space bounded by the two plates; but we have no experimental evidence that it extends in any other direction besides that thus determined by the position of the conducting plate. It becomes a matter of importance to determine this point, that is, whether the electrical condition of the liquid is the same in every direction around the zinc point, as it is in the direction of the copper plate; or whether the direction of its influence is determined *solely* by the position of the copper; and is entirely restricted to the mass of liquid interposed between it and the zinc, a question which appears to me to be finally determined by the investigations in sections 8 and 9.

165. 2nd inference. That in the fact of this divergence, or this diffusion of the electrical influence over a space progressively increasing with the distance, we have afforded the means of explaining the decrease in amount of action which follows an increase in the relative distance of the two elementary plates, on a principle much more probably true than that to which this phenomenon has generally been hitherto referred. It has generally been considered that this decrease is owing to the inferior conducting power of the greater mass of liquid interposed between the two plates.

166. 3rd inference. From the above results we must conclude, that the extent of this divergence is affected, in a certain degree, by the particular strength of the acid mixture in which it takes place; for although every condition except that of the strength of the acid was maintained uniform in the two sets of experiments above, yet the dimensions of the plates, and the positions at which they are needed to produce the same effect, are different in each. The amount of this difference is not great, nor can it be expected to be great; but it is sufficiently marked to indicate that such an effect does result from a difference in the particular strength of the mixture. But whether its immediate influence be due to the greater or less activity of the generating agents, or to the greater or less density of the acid mixture, or to whatever other cause, does not for the present appear.

167. 4th inference. That the amount of voltaic action at certain points within such arrangements undergoes a peculiar change, which, in the present state of our knowledge of its operations generally, is equally unexpected and inexpli-

cable. In some instances above, its amount is increased at certain positions, in others diminished, in cases in which, as yet, no apparent cause can be assigned for such an alteration. But the recurrence of this phænomenon, with a certain degree of regularity as to the positions at which it takes place, indicates that it has its origin in one uniform cause, the nature of which, however, so far, is wholly undetected.

168. 5th inference. That in this alternation in the amount of action, which is detected by the larger plates used in the latter experiments to occur at several positions as we recede from the zinc or generating point, we perceive an effect precisely similar, and in all probability identical with that already detected in the former experiments upon the effects of distance, as they were registered in tables, Nos. 5 and 6. The recurrence of this alteration at similar positions in both cases, indicates that in the several instances it has its origin in one common cause, whatever that cause may be.

169. I would not omit at this moment to remind you of the extent to which I consider myself indebted for a suggestion, which, in no inconsiderable degree, has influenced the reasoning upon some of the facts derived from the previous experiments, though that suggestion did not originate the experiments themselves. You may remember that some time past, whilst urging me to follow up some former inquiries, you hinted it as your opinion that some phænomena detected in them would prove ultimately to be due to radiation.

Although this was the extent of your communication, and I was alike ignorant upon what grounds such a conjecture had been founded, and whether or not you had entered upon any investigations in accordance with it, yet an idea at once so novel and elegant could not fail to exercise its due influence upon any one then engaged in such pursuits.

I am not prepared to say how far my own course of experimenting already devised might have led to a similar train of reasoning, since it is impossible that the mind could have been divested of the influence of such an impression thus incidentally conveyed.

I need not say how fully I shall feel recompensed for the labour of experiment, if the results thus obtained independently, shall be found in the end to be in accordance with your own views of the nature and extent of this phænomenon, or shall agree with the results of any investigations of it upon which you may yourself have entered.

170. My own results upon this point seem finally to indicate that this tendency to diverge is limited both in its extent

and direction ; and follows as a consequence of that law of action of voltaic arrangements stated in the previous part of this paper (section 1, 38, et seq.).

171. I find that the electrical influence developed in such single arrangements, diffuses itself from the generating to the conducting metal to such an extent, and in such a manner, that it occupies upon the surface of the latter a space exactly thirty-two times greater than that engaged upon the former. This is a constant result in every such arrangement ; and appears to depend upon the physical and chemical properties of the electrolyte ; and to be entirely independent of the kind of metal employed in the arrangement.

When the two metallic surfaces are in this proportion, then this diffusion upon the conducting surface, and the amount of action obtained in the arrangement, are at their maximum ; and when the conducting surface is made greater than this, then a singular change takes place, not only in degree (which is less), but also in the kind of action which results (see section 9).

I find that the direction in which this divergence takes place is determined entirely by the position of the conducting surface. This surface may be obtained by a *plate* of metal placed on one side of the generating point, or it may be so distributed as to be placed over against two or more of its sides, or it may be in the form of a *hollow cylinder* or *sphere*, and be distributed entirely around the generating point ; but whatever may be its direction with relation to that point, or howsoever distributed, the direction in which this divergence from that point takes place is determined accordingly, and the amount of action resulting exactly the same.

172. These effects are rendered obvious when the generating point consists of a small solid sphere of zinc, and the conducting surface, first of a flat plate of copper thirty-two times larger and placed on one side ; and then of an equal extent of surface distributed as a hollow sphere entirely around the zinc. By both is precisely the same amount of action obtained. By making either the plate or the sphere larger, we obtain no increase whatever of that amount ; but by reducing the extent of surface either of the sphere or the plate below this maximum size, then that amount is diminished in proportion and by the same rate in both cases.

173. But there are other influences affecting the distribution of the electricity when once generated in such arrangements. Some facts detected by experiment lead me to conclude, that the direction which the electricity takes between

the two elementary plates of any arrangement may be changed ; the nature of which change, and the circumstances under which it takes place, I proceed immediately to examine in the succeeding section.

[To be continued.]

XXXIX. *Experimental Researches in Electricity*.—*Eleventh Series*. By MICHAEL FARADAY, Esq., D.C.L. F.R.S. *Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.**

§. 18. *On Induction*. ¶ i. *Induction an action of contiguous particles*. ¶ ii. *Absolute charge of matter*. ¶ iii. *Electrometer and inductive apparatus employed*. ¶ iv. *Induction in curved lines*. ¶ v. *Specific inductive capacity*. ¶ vi. *General results as to induction*.

¶ i. *Induction an action of contiguous particles*.

1161. **T**HE science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its overruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels, or even equals in importance that called *Induction*. It is of the most general influence in electrical phenomena, appearing to be concerned in every one of

• From the *Philosophical Transactions* for 1838, Part I.

them, and has in reality the character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phænomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions, upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state, though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action, and the possible subjugation of many dissimilar phænomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquefied, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement* of the particles consistent with the transfer of the elements of the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing ac-

tions do not interfere; and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of Epinus, Cavendish, Poisson, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951, 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances: and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view, not merely in the accordance of known facts with it, but in

the consequences which would flow from it if true ; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view ; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which, according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164, 949.), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependence of induction on the particles ; and though this, in the theory of Poisson and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever-present question on my mind has been, whether electricity has an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge ; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attempts, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connexion with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting,

be charged with one electric force independently of the other, in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of Coulomb upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces*, are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electrics or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and, being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connexion with the wires, and supplied in every direction with bands of tin-foil, that the whole might be brought into good metallic com-

* *Mémoires de l'Académie*, 1786, pp. 67, 69, 72; 1787, p. 452.

munication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube above six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290.) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation), and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171.), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least signs of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the supposition of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the

other, and yet possible to have cases in which an isolated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids; these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.). The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, &c. &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negative result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only relatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All phænomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phænomena of electricity.

¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly as to leave no doubt of the results behind.

1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of Coulomb, constructed, generally, according to his instructions*, but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. Ritchie†. It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions, or 1440° , and yet, when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0.3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to Coulomb's direction, the arm carrying the ball being 2.4 inches long, and the other only 1.2 inches: to this was attached the vane, also described by Coulomb, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it, at the distance of 0.4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash

* *Mémoires de l'Académie*, 1785, p. 570.

† *Phil. Trans.*, 1830.

placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to Coulomb's method: the stem was fixed at its upper end in a block or vice, supported on three short feet: and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When, therefore, the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of 90° ; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusting screws on which the whole instrument stands, the glass torsion thread could be brought accurately into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of 90° was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of

the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in Coulomb's original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181.) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to 0° on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at 30° , and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring 0° upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of 30° being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of 30° between the centres of the balls was adopted, and found abundantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible by diminishing this distance was at perfect command; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The Coulomb balance electrometer requires experience to be understood; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of 400° at the standard distance of 30° , it took nearly four hours to sink to 50° at the same distance; the average loss from 400° to 300° being at the rate of $2^\circ.7$ per minute, from 300° to 200° of $1^\circ.7$ per minute, from 200° to 100° of $1^\circ.3$ per minute, and from 100° to 50° of $0^\circ.87$ per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducing surfaces of the conductors should have a constant form and state, and be at a constant distance from each other; and that either solids, fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it; the interval between the two being the space through which the induction was to take place. A section of it is given (fig. 1.) on a scale of one third: *a, a*, are the two halves of a brass sphere, with an air-tight joint at *b*, like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d*, which is itself attached either to the metallic foot *e*, or to an air pump. The aperture within the hemisphere at *f* is very small: *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball and its stem passes; *h* is the inner ball, also of brass; it screws on to a brass stem *i*, terminated above by a brass ball *B*; *l, l* is a mass of shell lac, moulded carefully on to *i*, and serving both to support and insulate it and its balls *h, B*. The shell-lac stem *l* is fitted into the socket *g*, by a little ordinary resinous cement, more fusible than shell-lac, applied at *m m* in such a

way as to give sufficient strength and render the apparatus airtight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball *h* and the surrounding sphere *a, a*. The ball *h* has a small aperture at *n*, so that when the apparatus is exhausted of one gas and filled with another, the ball *h* may itself also be exhausted and filled, that no variation of the gas in the interval *o* may occur during the course of an experiment.

1189. It will be unnecessary to give the dimensions of all the parts, since the drawing is to a scale of one third: the inner ball has a diameter of 2.33 inches, and the surrounding sphere an internal diameter of 3.57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0.62 of an inch; and the extent of this place or plate, i. e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball *h* and sphere *a, a*; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire *i*, so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by scale in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar *g* was made as thin as could be, that the lac might be as large there as possible. In order that at every

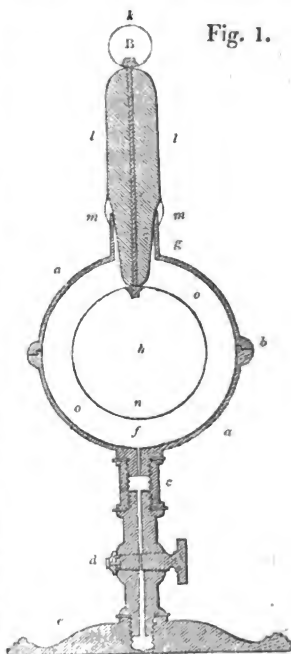


Fig. 1.

re-attachment of the stem to the upper hemisphere the ball *h* might have the same relative position, a gauge *p* (fig. 2.) was made of wood, and this being applied to the ball and hemisphere whilst the cement at *m* was still soft, the bearings of the ball at *q q*, and the hemisphere at *r r*, were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186.), i.e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top *k* of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then in falling from 600° to 400° the average loss was $8^\circ.6$ per minute; from 400° to 300° the average loss was $2^\circ.6$ per minute; from 300° to 200° it was $1^\circ.7$ per minute; from 200° to 170° it was 1° per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

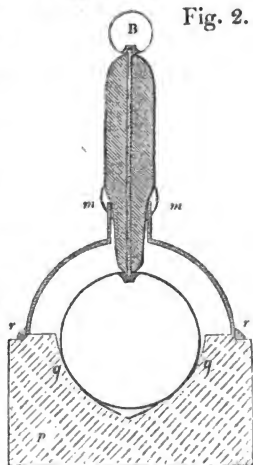


Fig. 2.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above 212° , air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *h* and *B*, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178.). Now though the largest portion of this induction is between the ball *h* and the surrounding sphere *a a*, yet the wire *i* and the ball *B* determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that respect remain the same, whilst the medium within at *o o*, may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184.), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one-sixteenth or one-twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is

to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of 30° by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls*.

1198. One of the inductive apparatus, as for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k*, fig. 1.), then introduced into the electrometer, and the repulsive force (at the distance of 30°) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics*.

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation purposely, to the

amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232.). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated*: the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experi-

ments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a perfect and instantaneous discharge, giving them no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

| App. i. | App. ii. |
|------------|----------|
| Balls 160° | |
| | 0° |

| App. i. | App. ii. |
|-----------------------------|---------------------------|
| 254° | |
| 250 | |
| divided and instantly taken | |
| 124 | 122 |
| 1 | after being discharged. |
| | 2 after being discharged. |

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischargable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after the division was found with 122°, this amount *at least* may be taken as what it had received. On the other hand 124° minus 1°, or 123°, may be taken as the half of the transferable charge retained by app. i. Now these do not differ much from each other, or from 124°·5, the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between 254° and 250° of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

| App. i. | Balls | App. ii. |
|-----------------------------|-------|--------------------------------|
| | 150° | |
| | | 152° |
| | | 148 |
| divided and instantly taken | | |
| 70° | | |
| | | 78 |
| | | 5 immediately after discharge. |
| 0 | | immediately after discharge. |

1211. The transferable charge being 148°—5°, its half is 71°·5, which is not far removed from 70°, the half charge of i.; or from 73°, the half charge of ii.: these half charges again making up the sum of 143°, or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from 0.62 to 0.435 of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of 469° was divided with app. i., the former retained a charge of 225° , whilst the latter showed one of 227° , i. e. the former had lost 244° in communicating 227° to the latter: on the other hand, when app. i. had a transferable charge in it of 381° divided by contact with app. ii., it lost 181° only, whilst it gave to app. ii. as many as 194° :—the sum of the divided forces being in the first instance *less*, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

[To be continued.]

XL. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 233.]

May 23.—A memoir entitled, a Synopsis of the English series of stratified rocks inferior to the old red sandstone—with an attempt to determine the successive natural groups and formations, by the Rev. Adam Sedgwick, *Woodwardian Professor in the University of Cambridge*, commenced on the 21st March, was concluded.

Introduction.—The author, after stating what was now offered to the Society to be only a first approximation, involving many questions of difficulty and doubt, pointed out the principles on which he had undertaken the task. There are two elements of classification applicable to stratified rocks of all ages, viz., physical structure and order of superposition; one giving the mineralogical unity of a group of rocks, the other their relative age. In addition to the two former, are classifications founded on the organic remains in the several groups. In the commencement of geology the last method was only subsidiary to the two former. But after observations had

been multiplied, laws respecting the distribution of organic types were discovered, which not merely superseded, in many large formations, all classifications founded on mineral structure; but often, through wide regions, gave indications of succession which were unsupported by the direct evidence of sections. As, however, the (so called) laws respecting the distribution of organic types, are mere general results grounded on actual observation, it is obvious that they can never upset conclusions drawn from the clear and unambiguous evidence of sections. The two methods may be used independently, and conspire to the same end; but in their nature cannot come into permanent collision.

The author then points out some examples in which these principles had been violated. (1) The attempt formerly made by some geologists to arrange the Stonesfield slate in a tertiary group, merely from the presence of certain fossils of a class not commonly found in secondary rocks. (2) Some of the doctrines put forth in the papers of M. Deshayes, which if pushed to their utmost extent would make the evidence of sections of no value; whereas without sections fossils could never have led to any general laws of succession. (3) The recent discussions respecting the age of the culm plants of North Devon. The plants were assumed to be of the age of the greywacké, from the mineral structure of the rocks in which they were imbedded; or the rocks were assumed to be of the carboniferous period by the species of the imbedded plants: whereas true geological reasoning required that, anterior to either of the preceding conclusions, the true position of the culm measures should be determined by actual sections.

The author then goes on to point out the difficulty of classifying the vast series of schistose rocks below the old red sandstone—from the great resemblance of their mineral type—from the absence of well-defined beds of organic remains in many large regions—and from their entire disappearance in the last members of the descending series. The Silurian system is almost the only exception to this remark; and even this system is developed in many parts of England without any distinct succession of natural groups. The mineral type is on the whole much more uniform in the great series under notice than in the secondary system of England; but the frequent absence of organic remains, and of any succession of distinct groups, is compensated by the enormous scale of development, as shown in the natural sections: and the author concludes, that it is not by hypothetical views and analogies, or by maintaining one part of geological evidence at the expense of another; but by applying every kind of evidence in its proper place, and above all by actual surveys and detailed sections, that we can ever hope to bring into coordination the complicated phenomena of which he is only attempting to give a brief synopsis.

TWO CLASSES OF OLD STRATIFIED ROCKS, &c.

The author first notices the older stratified series of Scotland, and divides it into two classes.

(1.) The primary class (composed of gneiss, mica slate, quartz rock, &c. &c.,) is largely developed in the Highlands.

(2.) The second class (greywacké, greywacké slate, &c. &c.,) is also largely developed in the Lammermuir hills, and in the whole chain extending in the south of Scotland from St. Abb's Head to the Mull of Galloway. It is shown, partly on mineralogical characters, and partly on the evidence of sections, that the rocks of the former class are inferior to those of the latter. For a zone of slate rocks (the *roches chloriteuses et quartzzeuses* of Dr. Boué) is superior to the crystalline slates of the Grampians, and is (at least provisionally) placed on the same parallel with the earthy and mechanical slates of the Lammermuir chain. Both the preceding classes are shown to be inferior, and generally unconformable, to the old red sandstone; which in the northern part of Scotland was once grouped with the primary class; but in the geological map of Scotland is now put in its true place.

After giving a series of sections to connect the structure of the Lammermuir chain with the adjacent parts of the north of England, he then proceeds to describe, in general terms, the expansion of the rocks of the second class through various mountainous tracts of South Britain. The frontier chain of Scotland—the slaty series of the Cumbrian mountains—of North and South Wales—and of the whole region between the eastern side of Devon and the western end of Cornwall—as well as the slate rocks of some smaller unconnected tracts, are all referred to one great class, the highest group of which passes into the old red sandstone, while the lowest (where the development is complete) rests on the crystalline system of the first class. Independently of the direct evidence from detailed sections, the several regions are shown to be related; 1st, by a common physical structure; 2ndly, by organic remains; 3dly, by common lines of *strike*; tending to show that several disconnected tracts of wide extent, having partaken of the same accidents, were once probably connected and continuous deposits in a deep sea.

In illustration of these views he shows that the prevailing strike of the beds (as well as the prevailing direction of the anticlinal and synclinal lines) in the Lammermuir system, in the Cumbrian system, and in the system of all the highest chains of North Wales, is nearly N.E. and S.W. and he further shows that the actual impress was given to all these regions before the period of the old red sandstone. In Cornwall the average strike is about W.N.W., but gradually bends round to the E. and W., in which prevailing direction the rocks cross Devonshire. In the southern parts of the slate regions of South Wales the beds also have an east and west strike; and these parallel dislocations of Devonshire and South Wales are posterior to the carboniferous series and probably contemporaneous with one another. Where the two preceding systems of strike meet, the beds are thrown into inextricable confusion; and on the outskirts of Wales, and in the counties where the Silurian system has been most largely developed, the dislocations are too irregular and complicated to be reduced to any law. Lastly, he notices a system of dislocations

that have brought up a portion of the older rocks (of the class here described) at Dudley, on both sides of the Warwickshire coal field, and in Charnwood forest. At all these localities the strike is the same, and the lines of greatest movement are nearly parallel—all being about N.N.W. and S.S.E.; and all these movements belong to one epoch, having been completed *after* the deposition of the lower red sandstone, and *before* the period of the upper and gypseous marls. Hence we have three great systems of elevation, each marked by parallel lines of strike, and the three systems of strike indicating three distinct periods of elevation.

The author then points out the importance of such facts to the broad speculations of geology, as well as the limitations under which they are to be applied. The dynamical powers of elevation appear to have been employed in three principal forms. 1st. In gradually raising up ridges through large spaces of the earth's crust. These will explain the correspondence of strike through very extensive regions; and such elevations if continued beyond a certain limit must have produced longitudinal fissures and lines of volcanic vent. 2ndly. In the long-continued protrusion and eruption of igneous rocks along such lines of vent. 3dly. In local and partial eruptions and protrusions, producing valleys of elevation, local derangements, and other phenomena that terminate in ordinary volcanic action. Elevatory forces, when considered in this general way, explain the phenomena of strike—the parallelism of great contemporaneous elevations—as well as the exceptions to the rule of parallelism.

GROUPS OF THE CUMBRIAN SECTION, &c.

The author then commences the separation of the whole series of rocks of the *second class* into natural groups, founded on sections exhibited in the several districts above noticed; and after shortly discussing two sections connecting the Cheviot hills with the formations in the basin of the Tweed, he describes in some detail a transverse section through the whole system of the Cumbrian mountains, which exhibits the following groups in ascending order.

(1.) The group of Skiddaw Forest, &c., the lower part of which rests on the granite, and passes into a system of crystalline strata resembling the rocks of the first class; the upper part abounds in a fine dark glossy clay slate, interrupted here and there by beds of more mechanical structure. The whole is of great thickness, almost without calcareous matter, and without any trace of organic remains, and forms the mineral axis of the Cumbrian mountains.

(2.) A group essentially composed of quartzose and chloritic roofing slates alternating with mechanical beds of coarser structure, and also with innumerable igneous rocks (compact felspar, felspar porphyry, brecciated porphyries, &c. &c.) which partake of all the accidents of the slates. It is of enormous thickness, and rises into the highest mountains of the country; and though chiefly developed on the south side of the preceding group (No. 1), it also appears extensively on the north side of the lower group, which thus forms a mineral axis—a fact not yet noticed in any of the published geological maps.

Though abounding in calcareous matter, it has no organic remains. (*Lower Cambrian system*).

(3.) A great series, expanded through Westmoreland and parts of Lancashire and Yorkshire. It is based on calcareous slates, passing into limestone, and full of organic remains, and in its lower division are fine roofing slates, but less crystalline than those of the preceding group. Its upper division (not however separable by any very distinct zoological or mineralogical characters from the lower) abounds in arenaceous flagstone, coarse quartzose greywacké, coarse slates with imperfect cleavage, and not fit for use, and the series is incomplete, being cut off by the unconformable deposits of old red sandstone and carboniferous limestone. Distinct beds of limestone are almost wanting in this upper division, and organic remains are very rare, but they appear here and there in very thin bands among the coarse siliceous slates. Provisionally, the lower division is placed in the *Upper Cambrian system*, and the upper division in the *Silurian system*; but without being separable into any further clear subdivisions. This great group (No. 3.) does not appear on the north side of the mineral axis of Cumberland, as was represented in the early geological maps.

SECTIONS OF NORTH WALES, &c.

The author next discusses a series of sections illustrating the structure of North Wales. One is drawn from the Menai Straits, in a direction about E.S.E., so as to cross the Berwyn chain and end in the carboniferous series near Oswestry. The others are drawn from the Berwyn chain to different parts of the carboniferous limestone range on the north side of Denbighshire. The greater portion of the first section crosses the older beds (the *Cambrian System*) which strike towards the N.E. The other sections intersect the upper series (*Silurian System*) which strike towards the N.W., passing (in some places unconformably) round the beds of the older system. From a consideration of the whole evidence the rocks are grouped in the ascending order as follows.

(1.) Chlorite slate, quartz rock, and mica slate of Anglesea and Caernarvonshire. These are placed on the parallel of the first class; and nothing is discovered in the section that is perfectly analogous with the Skiddaw slate, or first Cumbrian group, above described.

(2.) The old slate series of Caernarvonshire and Merionethshire, alternating indefinitely with bands of porphyry and felspar rock; many parts absolutely identical in structure with the second Cumbrian group above-described. It is of enormous but unknown thickness, and is bent into great undulations, the anticlinal and synclinal lines of which are parallel to the strike of the chain. Through wide tracts of country it is without fossils; but at Snowdon and Glider Fawr, encrinites, corals, and one or two species of bivalves have been discovered in it. It ends with the calcareous beds which range from Bala to the neighbourhood of Dinas Mowddu. This is called the *Lower Cambrian System*.

(3.) The next group (the *Upper Cambrian System*) commences with

the fossiliferous beds of Bala, includes all the higher portion of the Berwyns, and all the slate rocks of South Wales which are below the Silurian System. Its slate beds are less crystalline, and its general structure is more mechanical, than the preceding group, and it contains incomparably more fossils, which (though there are many extensive portions of the group without fossils) are disseminated through the more calcareous beds in great abundance. Many of the fossils are identical in species with those of the lower division of the *Silurian System*, nor have the true distinctive zoological characters of the group been well ascertained.

In many parts of South Wales it is separated from the *Silurian System* by great faults and derangements of the strata, marked by a broad band of rotten non-fossiliferous schist. At the north end of the Berwyn chain it appears to pass by insensible gradations into the lower division of the Upper System (the *Caradoc Sandstone*).

(4.) The last natural group (the *Silurian System*). For all details respecting this system the author refers to the abstracts of Mr. Murchison's papers*, and to his forthcoming work. He then describes the sections :

(1.) East of the Berwyns, in which the *Caradoc Sandstone* is finely developed; containing the *Llandeilo flagstone* and other characteristic calcareous and shelly bands.

(2.) The sections north of the Berwyns, connecting Montgomeryshire with Denbighshire. The ascending series is described as follows :—

- (1.) A series of beds several thousand feet in thickness, and apparently forming a passage between the Upper Cambrian and lowest portion of the Silurian System.
- (2.) Bands of calcareous slate with numerous organic remains of the "*Caradoc Sandstone*."
- (3.) Series of flagstones, more or less calcareous, with many orthoceratites and two species of *cardiola*, overlaid by, and associated with, irregular masses of roofing slate with a transverse cleavage.
- (4.) Flagstones and rotten slates, many parts in an imperfect state of induration, and the whole surmounted by the Carboniferous Limestone.—Of the preceding section the lower part of No. 3. is identical with the series of Long Mountain in the Silurian sections of Mr. Murchison; but No. 4. is mineralogically unlike any thing he has described, although it has been found to contain some of the fossils of the *Upper Ludlow Rock*. It appears from these details that the Silurian System, although its subdivisions are obscure from the absence of the Wenlock and Ludlow limestones, is more fully developed than in the group (No. 3.) of the great Cumbrian section above described.

The author then briefly notices the slate rocks of Charnwood Forest, which he refers provisionally to the Upper Cambrian System ;

[* See Lond. and Edinb. Phil. Mag., vol. iii. p. 224 ; vol. iv. p. 159, 228, 370, 450 ; vol. v. p. 217 ; and vol. vi. p. 314, 376.]

but from the imperfection of the sections and the absence of organic remains, their exact place is not determined.

SECTION FROM THE NORTH TO THE SOUTH COAST OF DEVONSHIRE.

I. *North Devon section.*—For details the author refers to a paper by Mr. Murchison and himself, but enumerates the successive groups for the purpose of adding some remarks, and of connecting the system of Devon with that of Cornwall*. The ascending order is as follows :

- (1.) A series of coarse arenaceous slates, not noticed in the former paper.
- (2.) The calcareous slates of the river Lyn.
- (3.) The coarse red flagstones, &c., of Exmoor Forest, and of the coast to the east of Combe Martin.
- (4.) The calcareous slates and limestone bands of Ilfracombe.
- (5.) The contorted slate zone south of Ilfracombe.
- (6.) The calcareous slates and irregular masses of limestone between the preceding group and the culm measures.

The whole of the preceding series is placed in the Upper Cambrian System with the exception of the upper portion of No. 6., which is considered, both from its structure and its fossils, as near the doubtful limit between the Upper Cambrian and Lower Silurian Systems.

II. *Culm measures.*—This series is described (as in a former paper) to occupy a great trough, which ranges across the country in a direction bearing nearly east and west; on its north side overlying the preceding group (No. 6. of the North Devon section), and on its south side rising up to the granite of Dartmoor, or overlying the older slate system of Devonshire and Cornwall†. Its subdivisions are enumerated as in the former paper; and the author adds, that during the summer of 1837 he ascertained that the lower beds of the culm measures rest unconformably on a portion of the slate rocks in the north of Cornwall, near Launceston. On the contrary, in the cliffs near Barnstaple, the lower culm measures seem to graduate almost insensibly into the formation on which it rests. Hence (independently of all other evidence) it is clear that the slate rocks in the north of Cornwall are of an older epoch than the upper group of the North Devon section.

The author then considers the classification of the culm series, and states his opinion that the base of it is lower than the base of the ordinary English carboniferous series. The base line (in the former paper) was intentionally left in an ambiguous position; and the difficulty of the subject has been subsequently increased by the supposed discovery of some true carboniferous plants in the highest group (No. 6.) of the North Devon section. In the upper part of the culm measures all the fossil plants have been described as identical in species with plants of the carboniferous series; and hence (unless some

* See Proceedings of Geological Society, vol. ii. p. 556 et seq. [or L. & E. Phil. Mag. vol. xi. p. 311.]

† See Proceedings, vol. ii. p. 561. [L. & E. Phil. Mag. vol. xi. p. 315, 316.]
Phil. Mag. S. 3. Vol. 13. No. 82. Oct. 1838. X

conflicting evidence be discovered) the culm measures and common coal measures must continue to be placed on the same parallel.

Lastly, he states that, independently of any question of classification, the former paper by Mr. Murchison and himself first pointed out the following facts in the general structure of the county:—

(1.) 'That the Wavellite rock and culm limestone (of Barnstaple, &c.) were in position, structure, and fossils distinct from all the other calcareous groups of Devon.

(2.) That the same group was repeated over again with a reversed dip on the north side of Dartmoor, and entirely distinct from the calcareous slates of Cornwall, with which it had no analogy in structure or fossils.

(3.) That the Holcombe Rogus limestone was a part of the culm series.

(4.) That the culmiferous system was superior to all the slate rocks of Devonshire and Cornwall, and was overlaid by no older rock than the new red sandstone. Whereas before, the portion of the culm series near the granite had, from its metamorphic structure, been confounded with the oldest rocks of Devonshire and Cornwall; and the position of whole series among the Devonian groups had been misapprehended.

III. *South Devon section.*—This section, in conformity with the scheme given in the former paper, is as follows, in the ascending order* :—

(1.) A series of slate rocks subdivided into two groups,—the lower containing a few calcareous bands, the upper group more calcareous and ending with the Plymouth limestone. The two are considered as one formation; and the name, *Ashburton bands*, which had been given to the calcareous beds of the lower division, is now withdrawn, as the position of the Ashburton lime rock is considered ambiguous. The name of *Ugborough bands* is not liable to the same objection.

(2.) A great group of coarse red flagstone and slate, identical in its structure with No. 3. of the North Devon section, and containing some corals that do not appear in the mountain limestone, but are found both in the Cambrian and Silurian systems. This group is provisionally identified with No. 3. of the North Devon section.

(3.) A great group of slate rocks without beds of limestone, and very rarely with any traces of organic remains. By the suppression of No. 4., this group is considered as the equivalent of No. 5. of the North Devon section.

(4.) Mica and chlorite slate, anomalous in structure and position, and forming no part of the ascending series.

The preceding identifications are only provisional, and many desiderata are enumerated; but it is considered certain that the South Devon section belongs, on the whole, to a lower series than the North Devon. Neither of them are, however, supposed to descend lower than the Upper Cambrian, or the higher part of the Lower Cambrian, group. To place the South Devon section above the North

* See Proceedings, vol. ii. p. 562. [or L. & E. Phil. Mag. vol. xi. p. 316, 317.]

Devon, would be to violate all the analogies of structure derived from other parts of England; and would not, the author believes, be supported by any specific evidence derived from fossils.

CLASSIFICATION OF THE ROCKS OF CORNWALL.

The author states, that the Plymouth limestone, in its range westwards, gradually thins off, and comes to an edge about the middle of Whitesand bay. The strike of the beds and the trending of the coast prevent this limestone and all the upper groups of the *South Devon section* from appearing again on the south-eastern side of Cornwall.

The inferior portion of the first group (No. 1.) of the South Devon section passes into Cornwall in a broad zone, gradually acquires the strike of the Cornish rocks, and so runs along the S.E. coast; and finally passes from Falmouth bay to Mounts bay; rising on its north side towards the granite, and on its south side dipping under the serpentine of the Lizard district. As in Devonshire, the group contains beds more or less calcareous, and, rarely, thin beds of limestone.

In the same way, though not with the same clear evidence, the calcareous slates rising from beneath the culm-measures near Launceston, double round the granitic promontory of Rough-Tor, and are thence expanded (though with considerable irregularities of strike and modifications of structure) as far as St. Ives' Bay.

The granitic ridge of the county is supposed to represent an interrupted mineral axis, on the N.E. and S.W. sides of which are slaty groups of the same geological period. In all cases near the granite the slaty groups change their structure; but this change of structure cannot be assumed as the ground of a classification dependent on the age of the deposit; as it is shown by a series of sections, that in several places the fossiliferous slates on the coast are of the same date with the indurated metalliferous slates that rise to the granite. Hence the crystalline and metalliferous slates of Cornwall are considered as metamorphic, and in that respect agree with the bottom culm series that touches on the Dartmoor granite.

Of the rocks of Cornwall the newest are the granites; next come the serpentine and other trappean rocks; and the oldest are the slate rocks. These slate rocks (including all the killas of Cornwall of whatever structure) appear to be an actual prolongation of the lowest group of the *South Devon section*, and therefore, agreeably to what is stated above, are provisionally arranged near the upper portion of the *Lower Cambrian System*.

Many of these rocks were formerly considered primitive; but none of them have any pretension to that class. Numerous fossils were found by the author in the cliffs on both sides Loe bay, and on both sides of the Fowey river, and still further west in Gerrans bay. The Rev. J. J. Conybeare found fossils many years since in the Tintagel slates; and the author in 1828 traced the fossiliferous system into the cliffs west of Padstow. During M. De la Beche's survey he had (before the author's last visit to the N.W. coast of Cornwall) found fossils innumerable in that part of the county. The Cornish fossils are

generally ill preserved; but among them are some corals that are common both to the Silurian and Cambrian systems. The fossils of New Quay and South Petherwin are an exception to the remark; as many of them are well preserved. They consist of corals; encrinurites; numerous specimens of the genera *Terebratula*, *Orthis*, and *Spirifer*; of four or five species of *Orthoceratites*; *Goniatites*; and lastly, three or four new species of a genus described by Count Munster under the name *Clymene*, and by Mr. Ansted under the name *Endosiphonites*. As they occupy a position so much lower, so, as a group, these fossils are distinct from those of the Silurian system.

Conclusion.—The author here takes a retrospect of the preceding description, and states that the classifications are founded on the details of actual sections; and that as far as such detailed sections throw light on the several questions that may arise, there is not much that remains to be done in England. Some of the generalizations are, however, founded on imperfect evidence; and to render them more complete, it is now necessary to appeal to the organic remains in the several groups. In this department little has been yet effected, excepting in the higher part of the Silurian system, where the upper divisions (at least in one part of the island) assume definite mineralogical and zoological types. Whether definite zoological groups can be made out in any lower system still remains to be seen. The rigid determination of the Devon and Cornish fossils, which are very numerous, and a rigid comparison of the Berwyn and Bala fossils with those near the base of the third group of the Cambrian section, give the fairest promise of an answer to the question, and are pointed out as immediate *desiderata*.

The difficulty of classification by organic remains increases as we descend, and is at length insurmountable; for in the lowest stratified groups, independently of metamorphic structure, all traces of fossils gradually vanish; and the great range of certain species through numerous successive groups, and the very irregular distribution of fossils even in some of the more fossiliferous divisions, add greatly to the difficulties of establishing true definite groups even within the limits of our island. The difficulties are indefinitely increased in comparing the formations of remote continents. But these circumstances are compensated by the magnificent scale of development of the successive groups, and their wide geographical distribution. Taken together they have a great unity of character; and even in remote continents they seem to form a common base, from which we may hope to compute the whole series of secondary and tertiary deposits that surmount them.

The author then briefly touches on questions of structure and cleavage; on the indefinite alternations of trappean beds; on metamorphic structure; on the long duration of the deposits; and on their great disruptions and symmetrical dislocations, indicating a greater violence of disturbing forces than is indicated in the secondary formations of this country. Following the geological scale of deposits from top to bottom, we can trace a series of phenomena indicating the same kind of causation differing at different times in intensity

and degree. The mean intensity can therefore only be collected by ascertaining the intensity during every geological period, and can never be obtained by assuming the intensity of any one epoch, past or present, as the arbitrary standard. Again, the successive organic types indicate great physical changes; and following the descending scale they at length vanish; conducting us, however, to the confines of other investigations in exact science which must prove the ultimate basis of physical geology.

Finally, the author gives a tabular arrangement of the several classes and subdivisions agreeably to the system of the preceding communication.

Class I.—Primary stratified Groups.

Gneiss, mica slate, &c., &c. Highlands of Scotland and the Hebrides. Crystalline slates of Anglesea and the S.W. coast of Carnarvonshire.

The series generally without organic remains; but should organic remains appear unequivocally in any parts of this class, they may be described as the *Protozoic system*.

Class 1. (a.) The crystalline slates of central Skiddaw forest, and the upper Skiddaw slate series. The whole is inorganic and intermediate between Class I. and Class II.

Class II., or Palæozoic series.

This class includes all the groups of formations between Class I. and the old red sandstone; and is subdivided as follows:—

1. *Lower Cambrian System.*—All the Welsh series under the Bala limestone. The two great groups of green roofing slate and porphyry on the north and south side of the mineral axis of the Cumbrian mountains. A small part of the slates of Cornwall and South Devon. A part of the slate series of the Isle of Man, &c., &c.
2. *Upper Cambrian System.*—A large part of the Lammermuir chain on the south frontier of Scotland. A part of the third Cumbrian group, commencing with the calcareous slates of Coniston and Windermere. The system of the Berwyns and South Wales. The slates of Charnwood forest. All the North Devon and a part of the South Devon series. The greater part of the Cornish series.
3. *The Silurian System.*—The upper part of the third Cumbrian group, chiefly expanded in Westmoreland and Yorkshire. The flagstone series of Denbighshire. The hills on both sides of Llangollen. The region east of the Berwyn chain. The regions described in the papers of Mr. Murchison, from which the types of the system are derived. The lowest part of the culmiferous series.

Over all the preceding comes the *Old Red Sandstone*—divided into three great natural groups in the country bordering the Silurian types of Mr. Murchison; in the northern counties developed in a less distinct manner, chiefly in the form of great unconformable masses of conglomerate, appearing at irregular intervals between the preceding groups and the carboniferous series.

Little notice is taken in the memoir of the crystalline unstratified rocks associated with the several series. Any questions of classification, bearing on their geological epoch, can only be determined by the effects, produced by them on the stratified series, which mark the period of their first protrusion; but for the present this subject is not touched on by the author.

XLI. Intelligence and Miscellaneous Articles.

CONTINUATION OF THE SCIENTIFIC MEMOIRS.

WE are glad to be enabled to announce that sufficient support has been proffered by private individuals and public bodies, to secure the continuation of the Scientific Memoirs; and that the FIFTH PART, being the commencement of a Second Volume, is in the course of preparation.

SYNAPTASIN.

M. Robiquet has given the name of *synaptase* to the principle of almonds, which possesses the singular property of reacting on amygdalin, and of determining, under the influence of moisture, the production of the oil of bitter almonds. The name of synaptasin is derived from the power of reuniting, as a connecting link, amygdalin and water. This substance possesses the following properties: it is of a yellowish white colour, sometimes brittle, and possessing the appearance of a varnish like dried gluten; at other times it is opake and spongy, like sarcocol. It is very soluble in cold water, but nearly insoluble in alcohol; when heated to about 140° Fahr. it is coagulated, when in solution in water; it is not precipitated either by acids or by acetate of lead, but readily by tannin; it does not like diastase form a paste when heated in water to 140° Fahr.; upon amygdalin it acts strongly, even at 176° Fahr.; when the solution is heated in contact with the air, it readily suffers a very evident decomposition, it becomes every day more turbid, and acquires a fetid smell, and, after a time, a very abundant flocky precipitate is formed; when subjected to the action of heat, it tumefies, yields empyreumatic oil, and an acid which contains a little ammonia. This acidity induced M. Robiquet to suppose that it retained a little of the acetic acid used in preparing it; when, however, it is put into contact with concentrated sulphuric acid, it undergoes a kind of softening, but neither acetic nor sulphurous acid is disengaged; when a drop of tincture of iodine is added to a solution of synaptasin, a deep rose-red colour is produced, but without any precipitation.

Synaptasin is obtained by the following process: almonds which have been deprived of their oil by pressure, are to be mixed with twice their weight of pure water, and the mixture is to be gradually pressed. After two hours' maceration, the liquid is to be filtered, the albumen is precipitated by acetic acid, and after filtration the gum is to be separated by means of acetate of lead, and after again filtering, the excess of acetate of lead is to be separated quickly by hydrosulphuric acid, and the excess of hydrosulphuric acid is to be

got rid of by the air pump; the sulphuret is to be separated by the filter, and the synaptasin is to be precipitated by a sufficient quantity of alcohol. The sugar remains in solution, and the synaptasin after washing with alcohol is to be dried in vacuo.

Journal de Chimie Medicale.—July, 1838.

COMPOSITION OF THE BLOOD.

M. Lecanu states that the venous blood of man may be considered on an average as composed of

| | |
|---------------|----------|
| Serum..... | 869·1547 |
| Globules..... | 130·8453 |

1000·

Or of

| | | |
|----------------------------------|----------|---------|
| Water..... | 790·3707 | |
| Oxygen | } | 10·9800 |
| Azote | | |
| Carbonic Acid..... | | |
| Extractive matters | | |
| Phosphorised Fat | | |
| Cholestrine..... | | |
| Serolin | | |
| Free Oleic Acid | | |
| Free Margaric Acid | | |
| Hydrochlorate of Soda | | |
| Hydrochlorate of Potash | | |
| Hydrochlorate of Ammonia | | |
| Carbonate of Soda | | |
| Carbonate of Lime | | |
| Carbonate of Magnesia | | |
| Phosphate of Soda | | |
| Phosphate of Lime | | |
| Phosphate of Magnesia | | |
| Sulphate of Potash..... | | |
| Lactate of Soda | | |
| Salt of fixed fat Acid | | |
| Salts of volatile fat Acid | | |
| Yellow colouring matter | | |
| Albumen of the Serum | 67·8040 | |
| Globules | 130·8453 | |

1000·

The Globules are stated to be composed of

| | |
|-----------------|----------|
| Fibrin | 2·9480 |
| Hematosin | 2·2700 |
| Albumen..... | 125·6273 |

130·8453

An. de Chimie, 67-64.

ON THE IODIDE OF AMIDIN. BY M. J. L. LASSAIGNE.

In 1833 the name of iodide of amidin was given by M. Lassaigne to the compound of iodine and the internal and soluble portion of fecula; the properties of this singular compound were then stated, and the decolorating power of heat was mentioned.

The results obtained by M. Lassaigne induced him to believe that this compound of iodine and amidin was soluble in water, contrary to the opinion of some chemists, who, subsequently to his experiments, endeavoured to show that this blue compound is merely suspended in the liquid in a state of extreme division. The observations made by M. Lassaigne are confirmed by those which he has since made:

1st. A solution of iodide of amidin prepared in July 1833, by pouring a solution of iodine into the soluble portion of fecula, obtained by treating the bruised grains with cold water, was placed in a dark closet. This solution, examined monthly to the present time during four years, has not yielded any deposit; it has always had the appearance of an homogeneous solution, equally coloured throughout of a fine indigo blue; and it has always had the appearance of freshly prepared iodide of amidin. The long time which this solution has been kept, without any sensible diminution of intensity, proves that it exhibits all the properties of a true combination; for a simple solution of amidin in water underwent a complete decomposition in some weeks, or at any rate it lost the property of colouring a solution of iodine blue.

2nd. The action of cold upon the above-described solution of iodide of amidin corroborates the opinion which has been expressed of its nature. It was exposed in the winter to a temperature of about 8° Fahr.; it solidified and became of a black-blue. During one night in January in which the temperature was about 10° Fahr., it was exposed to the air, and became a solid mass, of a brownish yellow colour, which it lost as the temperature became higher, and returned to a deep blue; placed in a warm room, it gradually liquefied, and during this change the iodide of amidin was deposited in flocks at the bottom of the bottle, the water remaining colourless.

This coagulation of the iodide of amidin by the action of cold, and its separation from the water which held it in solution before congelation, is attributed by M. Lassaigne to the cohesion of its molecules, which modified its affinity for water; by heating this fluid, in which the iodine was suspended and not dissolved, to about 60° Fahr., it re-dissolved and formed a fine blue coloured solution, and possessed all the characters which it had before congelation.

This observation, added to the former, leaves no doubt that iodide of amidin is really soluble in water at common temperatures, and that it evidently separates when a physical cause produces the approach of its molecules, or a chemical action determines its union with other bodies, which then render it insoluble in water.

It was found that a freshly prepared solution of iodine did not assume the brownish yellow colour, which the long prepared solution did when exposed to a low temperature. This difference is attri-

buted by M. Lassaigne to some change which had probably occurred in the nature of the long-kept solution.

Journal de Chimie Médicale.—May, 1838.

NEW COMPOUND OF SULPHATE OF MAGNESIA AND WATER.

M. Fritzche states that when a concentrated solution of sulphate of magnesia is exposed to the temperature of freezing water, there soon form, in the midst of small lamellar crystals of ice, a salt white as enamel and in smaller or larger crystals, according as the quantity of the solution is small or great. When large masses of this solution are allowed to cool during the winter, the salt often separates in crystals of a finger's length, and by gently thawing the liquid, they may be separated, for they undergo no change in water at 32°.

The enamel-white appearance which these crystals exhibit, arises from their consisting of a great number of still smaller crystals; the distinct crystals obtained by this process on the large scale have not the enamel tint, but are limpid and transparent. When subjected to a temperature above 32°, this compound soon begins to decompose; water separates; the crystals become opaque, and common sulphate is obtained with seven atoms of water. The new crystals retain their form, but the interior contains small crystals of the common sulphate. These crystals could not be dried even between folds of blotting paper without losing some water, and become slightly opaque at the surface: submitted to analysis the crystals were found to consist very nearly of

| | |
|---------------------------------------|-------------|
| One eq. of sulphate of magnesia | 60 = 35·77 |
| Twelve eqs. of water | 108 = 64·23 |

168 100·

L'Institut, Fév. 1838.

ON CHLORETHERAL BY M. FELIX D'ARCET.

When rough chloride of hydrocarbon is distilled in a water bath at 122°, there is readily obtained a liquor which boils at 185° Fahr., and this is pure chloride of hydrocarbon; the distillation then soon stops, and there remains in the retort a liquid of an oily appearance, which does not begin to boil until heated to about 284° Fahr., and its boiling point rises to 324°, and then remains stationary.

This liquid is about one fourth or one fifth of the original product, according as the washing-bottles of the olefant gas have been cooled with greater or less care.

Several experiments yielded the same results, and they were perfectly identical.

When freed from all extraneous matter, this substance is an extremely fluid liquid, colourless, limpid, and free from the smell of chloride of hydrocarbon. It has however a peculiar sweetish, æthereal odour, resembling that of sweet oil of wine; it burns, when a taper is presented to it, with a green flame.

The density of its vapour is 4·930; and by analysis it yielded :

| | |
|---------------|------------|
| Hydrogen..... | 5·41 |
| Carbon..... | 34·45 |
| Oxygen..... | 10·80 |
| Chlorine..... | 49·34—100· |

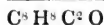
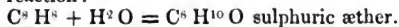
The formula according to M. D'Arcet is $H^8 C^8 O Cl^2$.

| | | Calculated. |
|--------------|----------|-------------|
| H^8 | 50..... | 5·5 |
| C^8 | 306..... | 34·7 |
| O..... | 100..... | 10·5 |
| Cl^2 | 442..... | 49·3 |
| | <hr/> | <hr/> |
| | 898 | 100· |

Excess of chlorine does not act upon this substance, or at any rate no new compound arises from their contact; this is also the case with ammonia.

This body appears to be, according to M. D'Arcet's nomenclature, the *chloral of æther*; except that, according to the law of substitutions, as the hydrogen which has disappeared, and which is replaced by chlorine, must belong to the water which constitutes the hydrate of the carburetted hydrogen of the æther, it ought not to be replaced.

Do not the following formulæ, inquires M. D'Arcet, appear to explain the reaction?



chlorethæral.

Annales de Chim. et de Phys., [xvi. 108.

[What is called the *law of substitutions*, about which no small parade is made by some foreign chemists, when stripped of its name means merely, I believe, what has been known ever since the doctrine of definite proportions was first established by Dr. Dalton, that when a definite quantity of any substance is displaced, that which replaces it is equally definite. I am not sure that I have rightly understood the author, and shall give a part of his statement in the original French: "Ce corps paraît être le chloral de l'éther; seulement, d'après la loi des substitutions, comme l'hydrogène qui a disparu, et qui a été remplacé par du chlore, doit appartenir à l'eau qui constitue l'hydrate d'hydrogène carboné de l'éther, il ne devait pas être remplacé."

The simplest view of the nature of this fluid is perhaps to consider it as an oxichloride of hydrocarbon, composed, according to the equivalents usually adopted in England, of,

| | |
|----------------------------|------------|
| Four eqs. of hydrogen..... | 4 or 5·5 |
| Four eqs. of carbon..... | 24 or 33·4 |
| One eq. of oxygen..... | 8 or 11·1 |
| One eq. of chlorine..... | 36 or 50· |

72 100·

[R. P.]

FORMIO-BENZOILIC ACID.

M. Laurent states when Nordhausen sulphuric acid is made to act upon the oil of bitter almonds, (hydruret of benzule,) they combine with the extrication of heat, and solidify into a fibrous mass. If water be poured upon it, two strata are formed, the lower one of which is acid, and the upper one oily. The oily stratum, which gradually solidifies, is a constant compound of $2 \text{ Bz} + \frac{3}{2} \text{ H}^{\circ} \text{ O}$; but it may present itself under two different and incompatible forms.

The liquid constituting the lower stratum, according to M. Laurent, is the *formio-benzoilic acid*. He says that it is formed at the expense of the hydrocyanic acid, which is decomposed by the influence of water and sulphuric acid, and gives rise to sulphate of ammonia and formic acid, which, being in the nascent state, combines with the hydruret of benzule to form the formio-benzoilic acid.—*Journal de Chimie Médicale*, November 1837.

PROPORTIONS OF GLUTEN IN GRAIN.

M. Boussingault has made researches on the proportions of gluten contained in the flour of different kinds of grain cultivated in the same soil.

He determined the quantity of gluten by ascertaining that of the ammonia which each yielded; this plan, it will be readily conceived, will yield much more precise results than that of working the flour between the fingers under a stream of water.

The flour obtained from different kinds of corn, but cultivated in the same soil, (that of the Jardin des Plantes,) yielded different proportions of gluten in the proportion of 15 to 21. The differences dependent upon the influence of the soil and that of the climate are much more strongly marked, and M. Boussingault has observed them to amount to from 1 to 4.—*Journal de Chimie Médicale*, November 1837.

OXIDE OF PHOSPHORUS.

M. Le Verrier proposes the following method of obtaining pure oxide of phosphorus, which he is of opinion has not been previously procured: take a glass globe, capable of holding about 2 pints, the neck of which is about 4 inches long and one inch wide; pour into this a little chloride of phosphorus, then introduce of phosphorus, previously dried on paper, and cut into pieces of about 8 grains each, enough to form a stratum of four-fifths of an inch thick, at the bottom of the globe: then add sufficient chloride of phosphorus to cover the phosphorus, and expose the whole to the air; 8 or 10 globes thus prepared are required to obtain 30 grains of oxide readily.

When about 24 hours have elapsed, a thick white crust of phosphatic acid is formed at the surface of the solution, whilst below the stratum of phosphorus there may be seen, through the glass, a yellow substance attached to it at the bottom of the globe; this is a compound of phosphoric acid and oxide of phosphorus, which the author calls *phosphate of oxide of phosphorus*.

In 24 hours after the appearance of the whitish matter, the quan-

tity of phosphate appears, in general, to be at its maximum. The chloride of phosphorus must then be poured off to make it serve for a fresh operation; the pieces of phosphorus which adhere together, at the bottom of the globe, must be detached, and gradually allowed to fall into cold water. By this proceeding, the considerable increase of temperature is avoided, which would otherwise occur, by a too rapid solution of the phosphoric acid, and of the excess of chloride of phosphorus; this would occasion the decomposition of the phosphate of oxide, as will presently appear. The water soon becomes of a deep yellow colour by dissolving the phosphate of oxide; and by decanting and filtering to free it from the suspended phosphorus, a perfectly limpid yellow liquid is obtained. By heating this solution, the phosphate of oxide decomposes at about 177° Fahrenheit into phosphoric acid, and a yellow, finely-divided, flocculent matter, which, however, collects pretty rapidly at the bottom of the water. This substance is hydrated phosphoric oxide, which is nearly insoluble in water. This hydrate may, in a short time, be washed upon a filter with hot water; but in order to have the product not soiled by the paper, it must not be dried upon the filter, but it must be removed from it, while moist, to a porcelain capsule, and dried, *in vacuo*, over sulphuric acid. The oxide not only loses the interposed water, but also that which it contained in combination: the hydrate is decomposed, and perfectly pure oxide of phosphorus remains; it has the form of small grains, which are of a red colour; but when finely powdered, it is canary yellow.

It was proved to contain no chlorine, by dissolving in nitric acid, and finding none in the solution, and it contained no hydrogen; for by burning with oxide of copper, it yielded no water. By converting this oxide into phosphoric acid, and that into phosphate of lead, it was found to consist, very nearly, of

| | | |
|-----------------|-----------------|-------|
| Oxygen..... | 11.35 or 1 eq. | 8 |
| Phosphorus | 88.65 or 4 eqs. | 64 |
| | <hr/> | <hr/> |
| | 100. | 72 |

This oxide is insoluble in water, alcohol and æther; its density is greater than that of water. At the moment of withdrawing it from the vacuum, it has neither smell nor taste, and it remains in this state, either in contact with the air or dry oxygen. But when these gases are moist, it slowly acidifies, yielding a slight odour of phosphuretted hydrogen. It is not luminous in the dark under any circumstances.

Out of contact with the air it may be kept at a temperature of about 570° without decomposing, but it becomes of a bright red colour; at a little below the heat of boiling mercury, it decomposes rapidly, phosphorus distils, and perfectly white phosphoric acid remains. When heated in the air, it remains unchanged at a high temperature, and it burns only when it disengages phosphorus. Chlorine converts it into chloride of phosphorus and phosphoric acid. Hydrochloric acid, whether gaseous or in solution, has no effect on this oxide; when heated with concentrated sulphuric acid, sul-

phurous acid is given out; nitric acid converts it into phosphoric acid.

When mixed with chlorate of potash it gives a fulminating powder, which detonates, sometimes, during the mixture, and without giving it any pressure; a slight pressure always occasions it to explode. The hydrate of phosphorus, which has been mentioned, decomposes *in vacuo*, or by exposure to the air at common temperatures. The quantity of water which it contains was determined by indirect processes to be 20·5 per cent., so that it is composed of one eq. of oxide $72 + 2$ eqs., water $18 = 90$, very nearly.—*Annales de Chimie et de Physique, Juillet 1837.*

BORATES OF POTASH.—BY M. LAURENT.

The borates of potash were prepared by decomposing a hot solution of carbonate of potash with excess of boracic acid. A part of the salt was made to crystallize, and to the remaining solution a little caustic potash was added, and crystallization was effected after each addition of the alkali. The crystals which were formed were kept separate and examined.

Sexborate of Potash is deposited from a solution which is either acid or neutral to litmus paper. The crystals belong to the right prismatic system (right rhombic prisms) and are generally modified on the edges or angles, and are occasionally tabular and thin. The forms are not readily perceived on account of the crystals being hemitrope.

This salt is unalterable in the air; very brilliant; it is but slightly soluble in cold water, but readily dissolved by boiling water; it is neutral to litmus paper, or rather it blues it slightly.

This salt was analyzed by passing gaseous hydrofluoric acid on the pulverized borate, moistened and placed in a platina crucible; the fluoride of potassium was converted into neutral sulphate by sulphuric acid, calcination, and the addition of carbonate of ammonia. The results of the analysis were—

| | By Experiment. | Calculated. |
|--------------------|----------------|-------------|
| Boracic Acid - - - | 60·8 | 60·5 |
| Potash - - - - - | 14·0 | 13·6 |
| Water - - - - - | 25·2 | 25·2 |

100·0

The calculation was made according to the following formula:—



Taking Berzelius's recent atomic weight of boracic acid as $\text{B}^2 \text{ O}^3$, it will be seen that this salt, which is rather alkaline than acid, contains, nevertheless, six atoms of acid.

Triborate of Soda. On gradually adding potash to the preceding salt, the liquor becomes alkaline, and by evaporation it deposits at first a confusedly crystalline crust, which appeared, however, to be sexborate, and the mother waters eventually yielded baborate. Sometimes, and especially at the surface of the solution, there are formed very distinct crystals presenting the form of rectangular

prisms, the terminal edges being replaced by planes that produce four-sided pyramids on each base.

| This salt consists of— | By Experiment. | Calculated. |
|------------------------|----------------|-------------|
| Boracic Acid - - - | 46·4 | 47·0 |
| Potash - - - - - | 21·6 | 21·0 |
| Water - - - - - | 32·0 | 32·0 |
| | <hr/> 100·0 | <hr/> 100·0 |

The formula is $3 B^3 O^3 + K O + 8 H^2 O$.

These crystals are unalterable in the air, and, like the preceding, they fuse readily, and swell but little.

Rhombic Biborate of Potash. This salt, the composition of which corresponds to that of octahedral borate of soda, possesses, nevertheless, a different and an incompatible form; it generally crystallizes in hexagonal prisms, and rarely as an acute rhomboid—primary form, an acute rhomboid occasionally with the lateral angles replaced, or as a bipyramidal dodecahedron or hexagonal prism.

This salt is alkaline, very soluble in cold and in boiling water; when fused it swells up like borax; it is composed of—

| | By Experiment. | Calculated. |
|--------------------|----------------|--------------|
| Boracic Acid - - - | 43·7 | 43·2 |
| Potash - - - - - | 28·5 | 29·5 |
| Water - - - - - | 27·8 | 27·8 |
| | <hr/> 100·0 | <hr/> 100·0* |

The formula is $2 B^3 O^3 + K O + 5 H^2 O$.

Sexborate of potash does not precipitate magnesia, oxide of manganese or oxide of silver, from solution; the following are the effects which it produces in the annexed metallic solutions:—

| | |
|-------------------|--|
| Barium | } A white precipitate, which disappears on the addition of excess of water; the solution becoming slightly alkaline. |
| Strontium | |
| Calcium | |
| Peroxide of iron, | a rusty yellow precipitate. |
| Copper, | bright blue ditto. |
| Nickel, | greenish. |
| Chromium, | green. |
| Lead, | white. |
| Silver, | white, which disappears on the addition of excess of water. |

An. de Ch. et de Ph. 67—215.

CYANIDE OF GOLD.

M. Defferre prepares this compound by dissolving 16 parts of gold, cut into small pieces, in 80 parts of aqua regia, heated in a sand-bath; to the solution there are to be added 24 parts of cyanide of mercury, dissolved in 24 parts of distilled water; the whole is to be evaporated to dryness, and the residue treated with 192

* There is a mistake here in the original; the substances amount to 100·5.

parts of distilled water; agitate the mixture, allow it to remain for some time, and then pour off the liquor from the cyanide of gold.

To the mother water add 8 parts of cyanide of mercury, and again evaporate to dryness; again add 192 parts of distilled water to the dry residue, and again agitate; suffer to remain, and pour off from the cyanide of gold; this may be repeated a third and a fourth time, or until no more cyanide of gold of a fine colour is produced; the operation with the mother water may be repeated without its being necessary to add cyanide of mercury every time.

All the cyanide of gold obtained ought to be afterwards washed with distilled water, till it comes away quite insipid, or until reagents show that it is entirely free from bichloride of mercury.

Every time the mother water is used, it should be slightly acidified with a few drops of aqua regia; without this the cyanide of gold which separates would acquire by evaporation a yellow reddish colour. During the evaporation to dryness on the sand-bath, the solution should be constantly stirred with a glass rod, till it acquires a bright canary yellow colour; the occurrence of this denotes the formation of the cyanide of gold, mixed with bichloride of mercury, and an excess of cyanide of mercury undecomposed; these are got rid of by washing with distilled water.

The new Codex recommends the employment of *pure* cyanide of potassium as being essential to the success of the operation; but the difficulty of procuring this salt in a pure state is well known. Besides, the instability of this salt, even when it is kept in well-stopped bottles, will always throw uncertainty on the results of the operation; an objection which does not attach to the use of cyanide of mercury.—*Journal de Pharmacie*, xxiv. p. 27.

METEOROLOGICAL OBSERVATIONS FOR AUGUST 1838.

Chiswick.—August 1. Very fine: heavy rain at night. 2. Rain. 3. Fine. 4. Overcast: slight rain. 5. Cloudy. 6. Sultry, with showers. 7. Showery. 8, 9. Fine. 10. Overcast. 11—16. Very fine. 17. Hazy: slight rain. 18—20. Very fine. 21. Showery. 22. Rain. 23. Showery. 24. Cloudy and fine. 25. Foggy: rain. 26, 27. Very fine. 28. Overcast: lightning at night. 29. Cloudy and fine. 30. Clear and dry. 31. Very fine.

Boston.—August 1. Fine. 2. Cloudy: rain early A.M. 3. Rain. 4. Fine: rain P.M. 5. Fine. 6. Cloudy: rain P.M. 7. Heavy rain with thunder and lightning P.M. 8. Cloudy. 9. Fine: rain P.M. 10. Cloudy: rain P.M. 11, 12. Cloudy. 13—15. Fine. 16. Cloudy. 17, 18. Fine. 19, 20. Windy. 21. Windy: rain early A.M.: rain P.M. 22. Windy: rain P.M. 23. Stormy. 24, 25. Fine. 26. Cloudy. 27. Cloudy: therm. 74° 6 P.M. 28. Cloudy. 29. Windy: rain early A.M. 30, 31. Fine.

Applegarth Manse, Dumfries-shire.—August 1. Rain P.M.: warm and moist. 2. Fine day throughout. 3. Fine day: occasional showers. 4. Moist and cloudy. 5. Heavy rain P.M. 6. Very heavy showers. 7. Showery all day. 8. Fine summer day: cool P.M. 9. Wet throughout. 10. Fair A.M.: wet evening. 11. Drizzling rain A.M.: fair P.M. 12. Fair: shower P.M. 13. Fair: shower at noon. 14. Fair throughout. 15. Fine clear day. 16. Temperate: cool. 17. Beautiful summer day. 18. Cloudy: moist P.M. 19, 20. Showery all day. 21. Rainy all day: flood. 22. Showery all day. 23, 24. Fair A.M.: shower P.M. 25. Very moist: rain P.M. 26. Fair: warm: cloudy. 27. Fair, but threatening. 28. Drizzling all day. 29. Clear and cool. 30. Temperate. 31. Mild though cloudy.

| Day of Month.
1888.
August. | Barometer. | | | | Thermometer. | | | | Wind. | | | | Rain. | | | | Dew-point.
London:
Roy. Soc.
9 a.m. | | | |
|-----------------------------------|--------------------------------|-------------------|--------|--------------------|---------------------------|---------------------------|--------------------------------|-------------------|-------|-------------------|-------|--------------------------------|----------------------------|-------|-----------------|--------------------------------|--|-----------|---------|-----------------|
| | London:
Roy. Soc.
9 a.m. | Chilwick.
Max. | Min. | Boston.
84 a.m. | Dumfries-shire.
9 a.m. | Dumfries-shire.
9 p.m. | Fahr. Self-register.
9 a.m. | Roy. Soc.
Max. | Min. | Chilwick.
Max. | Min. | London:
Roy. Soc.
9 a.m. | Chilwick.
E. & S. by E. | Boat. | Dumfries-shire. | London:
Roy. Soc.
9 a.m. | | Chilwick. | Boston. | Dumfries-shire. |
| 1. | 30.058 | 30.014 | 29.904 | 29.47 | 29.91 | 29.73 | 63.7 | 71.5 | 53.5 | 75 | 57 | 63 | 59 | 54 | calm | sw. | ... | .17 | ... | 56 |
| 2. | 29.844 | 29.844 | 29.743 | 29.17 | 29.58 | 29.45 | 66.6 | 71.0 | 59.2 | 69 | 56 | 66 | 60 | 53 | w. | sw. | .133 | .11 | .43 | 60 |
| 3. | 29.738 | 29.768 | 29.716 | 29.05 | 29.48 | 29.52 | 64.7 | 69.0 | 58.7 | 74 | 59 | 62 | 59 | 56 | calm | s. | .061 | ... | ... | 60 |
| 4. | 29.690 | 29.782 | 29.606 | 29.00 | 29.44 | 29.36 | 66.8 | 71.3 | 61.7 | 71 | 52 | 66.5 | 61 | 58 | w. | sw. | .013 | .08 | .09 | 62 |
| 5. | 29.574 | 29.564 | 29.531 | 28.95 | 29.36 | 29.28 | 63.3 | 70.2 | 58.0 | 72 | 56 | 62 | 60 | 56 | w. | s. | .063 | .12 | .03 | 59 |
| 6. | 29.524 | 29.592 | 29.507 | 28.86 | 29.25 | 29.32 | 63.8 | 68.7 | 58.8 | 70 | 52 | 64 | 56.1 | 56 | w. | sw. | .088 | .22 | ... | 60 |
| 7. | 29.732 | 29.971 | 29.682 | 29.03 | 29.53 | 29.77 | 62.5 | 63.0 | 55.0 | 70 | 50 | 57.5 | 58.4 | 55 | w. | sw. | .338 | .04 | .09 | 57 |
| 8. | 30.056 | 30.156 | 30.027 | 29.43 | 29.93 | 30.07 | 60.5 | 68.0 | 54.0 | 69 | 42 | 59 | 57.1 | 56 | calm | sw. | .066 | ... | .90 | 60 |
| 9. | 30.236 | 30.224 | 30.104 | 29.66 | 29.93 | 29.80 | 63.5 | 67.0 | 51.0 | 70 | 55 | 60 | 56 | 54 | w. | sw. | ... | .01 | ... | 58 |
| 10. | 30.074 | 30.118 | 30.039 | 29.40 | 29.78 | 29.74 | 63.8 | 69.0 | 58.0 | 73 | 60 | 66 | 58 | 59 | w. | sw. | ... | .06 | ... | 60 |
| 11. | 30.096 | 30.108 | 29.966 | 29.35 | 29.73 | 29.84 | 68.4 | 72.0 | 62.0 | 75 | 57 | 70 | 61 | 58 | w. | sw. | ... | ... | ... | 62 |
| 12. | 30.120 | 30.098 | 30.077 | 29.38 | 29.89 | 29.90 | 67.0 | 72.5 | 61.0 | 78 | 61 | 65 | 59 | 56 | w. | w. | ... | ... | ... | 63 |
| 13. | 30.094 | 30.222 | 30.075 | 29.42 | 29.90 | 30.07 | 65.0 | 75.6 | 71.0 | 76 | 41 | 63 | 57.1 | 53 | w. | w. | ... | ... | ... | 63 |
| 14. | 30.262 | 30.243 | 30.142 | 29.63 | 30.09 | 30.07 | 60.6 | 71.0 | 51.0 | 72 | 46 | 59 | 56.1 | 52 | w. | w. | ... | ... | ... | 56 |
| 15. | 30.158 | 30.191 | 30.134 | 29.56 | 30.09 | 30.10 | 61.2 | 68.5 | 53.0 | 71 | 39 | 59 | 59 | 50 | sw. | sw. | ... | ... | ... | 58 |
| 16. | 30.146 | 30.133 | 30.082 | 29.58 | 30.04 | 30.04 | 62.0 | 67.0 | 55.0 | 75 | 53 | 57.5 | 57 | 49 | sw. | w. | ... | ... | ... | 55 |
| 17. | 30.190 | 30.248 | 30.165 | 29.67 | 30.12 | 30.14 | 58.0 | 70.0 | 55.0 | 70 | 54 | 56 | 55 | 55 | sw. | sw. | ... | .01 | ... | 58 |
| 18. | 30.254 | 30.249 | 30.168 | 29.66 | 30.14 | 30.00 | 62.5 | 62.5 | 58.0 | 76 | 56 | 60 | 60 | 53 | sw. | sw. | .044 | ... | ... | 60 |
| 19. | 30.012 | 30.011 | 29.890 | 29.35 | 29.72 | 29.63 | 67.4 | 70.6 | 59.5 | 76 | 46 | 64.5 | 57 | 34 | w. | sw. | ... | ... | 2.33 | 62 |
| 20. | 29.832 | 29.819 | 29.481 | 29.22 | 29.53 | 29.27 | 61.2 | 73.0 | 60.6 | 71 | 56 | 62 | 56.1 | 53 | sw. | s. | .008 | .03 | ... | 57 |
| 21. | 29.358 | 29.398 | 29.325 | 28.68 | 29.04 | 28.96 | 65.5 | 69.0 | 59.5 | 72 | 51 | 62 | 55 | 53 | w. | sw. | ... | .09 | .04 | 60 |
| 22. | 29.252 | 29.313 | 29.249 | 28.60 | 28.83 | 29.17 | 60.2 | 69.0 | 54.2 | 66 | 50 | 58 | 54 | 53 | w. | sw. | .050 | .12 | .06 | 55 |
| 23. | 29.468 | 29.816 | 29.438 | 28.87 | 29.43 | 29.73 | 59.0 | 65.0 | 54.5 | 67 | 46 | 58 | 55 | 51 | sw. | sw. | .088 | .10 | .15 | 56 |
| 24. | 29.966 | 30.097 | 29.952 | 29.40 | 29.90 | 29.99 | 59.0 | 65.0 | 51.2 | 68 | 41 | 56.5 | 55 | 50 | sw. | sw. | .019 | .04 | .02 | 57 |
| 25. | 30.102 | 30.110 | 30.025 | 29.54 | 29.98 | 29.89 | 57.0 | 63.5 | 49.7 | 66 | 48 | 57 | 50.1 | 52 | sw. | sw. | ... | .09 | ... | 54 |
| 26. | 30.108 | 30.124 | 30.090 | 29.52 | 29.92 | 29.89 | 60.2 | 65.5 | 54.0 | 77 | 54 | 60 | 58 | 57 | sw. | sw. | .066 | ... | ... | 58 |
| 27. | 30.132 | 30.135 | 30.110 | 29.46 | 29.92 | 29.97 | 68.0 | 69.0 | 60.0 | 81 | 59 | 68 | 59 | 60 | sw. | sw. | .013 | ... | ... | 60 |
| 28. | 30.086 | 30.073 | 30.075 | 29.38 | 29.88 | 29.70 | 67.0 | 76.0 | 64.0 | 82 | 56 | 68.5 | 59.1 | 57 | sw. | sw. | ... | ... | ... | 65 |
| 29. | 29.830 | 30.175 | 29.784 | 29.22 | 29.94 | 30.08 | 62.4 | 76.0 | 61.0 | 67 | 39 | 56 | 54 | 49 | sw. | sw. | ... | .15 | ... | 60 |
| 30. | 30.190 | 30.197 | 30.154 | 29.60 | 30.08 | 30.10 | 55.4 | 62.5 | 46.5 | 71 | 38 | 54 | 52 | 55 | sw. | sw. | ... | ... | ... | 51 |
| 31. | 30.190 | 30.183 | 30.130 | 29.60 | 30.07 | 29.93 | 60.6 | 65.8 | 50.3 | 75 | 43 | 58 | 55 | 56 | sw. | sw. | ... | ... | ... | 55 |
| Mean. | 29.947 | 29.999 | 29.874 | 29.31 | 29.75 | 29.75 | 62.8 | 69.0 | 56.4 | 72.42 | 50.76 | 61.2 | 56.1 | 53 | ... | ... | Sum. | 1.23 | 2.05 | Mean.
58.6 |

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XLII. *On a Principle laid down by Clairaut for determining the Figure of Equilibrium of a Fluid, the Particles of which are urged by accelerating forces.* (Théorie de la Figure de la Terre, première partie, chap. 5^{me}.) By JAMES IVORY, K.H., F.R.S., &c.*

CLAIRAUT's principle alluded to, has all the precision and elegance which are admired in the applications of the ancient geometry, such as are found in the writings of Archimedes. It might not be useless to inquire how it happens that geometers, neglecting the elegant method of Clairaut, have so generally agreed in giving the preference to the theory of Euler, grounded on the equality of pressure which necessarily takes place in every fluid at rest, whatever be the causes by which such a state is induced. It would, no doubt, be found that this is owing to the great generality of the equations of Euler's method, which are easily obtained, and seem to embrace every proposed problem and to reduce it immediately to a question of abstract mathematics.

Clairaut begins with supposing a mass of fluid in equilibrium, thus laying a sound foundation for his reasoning. The forces at the several points of the supposed mass, which may be called A , being expressed in numbers, he next spreads over the surface of A , a thin stratum δA , so as to fulfil the condition, that the force at any point of the surface of A multiplied by the thickness of δA at the same point, shall constantly make the same product. This being done, when the quantities of the matter of the stratum on which the forces act are taken into account, it follows that every infinitesimal portion of the surface of A , will sustain a proportional pressure; so that the intensity of pressure, estimated on a given

* Communicated by the Author.

surface, is invariably the same over all the surface of A . Now the supposed equilibrium of A will not be disturbed by pressures of the same intensity exerted at all the points of its surface; and therefore a new body of fluid in equilibrium, namely, $A + \delta A$, is obtained. Continuing to reason in like manner, the original mass A may be enlarged to any dimensions by the addition of successive strata, at the same time that an equilibrium is preserved at every step. It is easy to make the procedure independent of the first supposition, that A is in equilibrium; for as the reasoning holds whether A is great or small, we may suppose it so small that any forces inherent in its own particles and tending to change its figure, are overpowered and annihilated by the accumulated pressure of the incumbent strata.

When the property of equilibrium, demonstrated in these few words, is put in equations as Clairaut has done, these equations are found to be the very same as those deduced from Euler's theory. Thus, in point of application, the two methods are entirely equivalent: if one is capable of solving a problem, the other may be used with equal success.

The investigation of Clairaut is clear and definite. It evidently assumes that there is no cause tending to disturb the equilibrium of A , except the action of the forces at the surface of A upon the matter of δA . On this account his method fails when there is a mutual attraction between the mass A and the stratum δA . If the mass A attract the matter of the stratum δA and cause it to press, it follows necessarily that the matter of δA will react, and, by its attraction, will urge the particles of A to move from their places. In this case therefore the equilibrium of A is disturbed by a cause which Clairaut has not attended to; and unless the effect of this new force is counteracted, the body of fluid $A + \delta A$, will not be in equilibrium. The principle of the method suggests a remedy for this omission; for it is easy to prove that the equilibrium of A will not be disturbed by the attraction of the stratum δA , if the resultant of that attraction upon every particle in the surface of A , be directed perpendicularly to that surface. And thus we arrive at the same two independent conditions for the equilibrium of a fluid consisting of attracting particles, which have been found necessary in every other way of solving the same problem when nothing essential is neglected. (Vide this Journal for August last, p. 81, and for October, p. 274.).

The principle of Clairaut's method, when enunciated generally, lies in this, that the supposed equilibrium of A is not to be disturbed by the addition of a stratum; and therefore

in applying it, every force which is introduced by a stratum and is capable of moving the particles of A, must be carefully ascertained and made ineffective.

The method of Clairaut, as it has been explained and extended, will help us to form a just notion of Euler's theory. A very little attention will show, that all the perplexities that have ever attended this theory, originate in misconception of the manner in which a particle of a fluid in equilibrium is made immoveable by the pressures which act upon it. If we conceive a particle placed on one of the interior surfaces in Clairaut's method, that is on a level surface, it is most evident that all the pressure upon the particle is caused by the action of the fluid above it, or on the outside of the level surface. If the fluid without the level surface were removed, there would be no pressure upon the particle, which would only be subject to the action of the forces inherent in the surface on which it is placed. Now pressures of equal intensity are impressed at all the points of a level surface; these pressures are transmitted through the contained fluid to the particles on the surface; so that every such particle is pressed equally in all directions by the action of the fluid exterior to the surface. It is therefore correct to say that a particle in a level surface is not moved by the pressures caused by the forces which urge all the fluid on the outside of that surface: but this does not demonstrate that the particle is at rest by the action of the whole mass. To complete the proof of an equilibrium, it is further necessary to show, that the body of fluid within the level surface is not liable to a change of its form or position by the forces that act on its own particles. The theory of Euler is therefore chargeable with mistaking the action of a part only of the fluid, for the effect of the forces that urge all the particles of the mass. If a canal be drawn in any manner from a particle in a level surface to terminate in the upper surface, it will invariably exert the same pressure upon the particle: but the nature of a level surface is such, that a part of any canal within it presses neither way; so that the pressure upon the particle is produced solely by the action of, the forces upon the part of a canal exterior to the level surface. We may now safely affirm that the great generality of Euler's theory arises from omitting what is essential to an equilibrium; and enough has been said to show that, when the omission is supplied, we arrive at the principles of Clairaut's method by a route which is different indeed, but not so direct.

One observation more it seems requisite to make. Euler has investigated this theory in a memoir (*Mem. de Berlin*,

1755) which cannot be enough admired for invention and all the qualities of the highest mathematical genius, if we overlook the too precipitate adoption of a principle seducing by its great generality. But the equality of pressure seems to have been first used in solving this problem by Maclaurin. If we strictly appreciate what that great geometer has proved in his celebrated demonstration, it will be found not only not inconsistent with what has been said, but to acquire force and simplicity when stated according to the foregoing principles. Taking any particle of the fluid spheroid, Maclaurin proves that any rectilineal canal standing upon it and terminating in the upper surface, urges it to move with a force equal to the effort of the fluid in the difference of the polar semi-axes of the proposed spheroid and a similar one, the surface of which passes through the particle. (Fluxions, § 639). Now this does not prove that the particle is at rest by the equal pressures upon it; but, on the contrary, that it may have any position on an elliptical surface without any variation of the pressures which urge it. It follows indeed that all the particles on the same elliptical surface are in equilibrium relatively to that surface, that is, there is no force urging them to move upon it; but, even when this is attended to, another condition is still wanting to prove the immobility of the particle relatively to the whole spheroid, which is, the stability, both in form and position, of the elliptical surface which contains the particle. These observations being duly weighed, it is obvious that all the inferences from what Maclaurin has proved, are deducible from the equality of pressure at all the points in the surface of every interior elliptical spheroid, concentric and similar to the given one; which is according to Clairaut's principle.

In his work on the Theory of the Earth, Clairaut has adopted the method of Maclaurin in preference to his own theory, of which he makes little use. In reality the equilibrium of a homogeneous planet in a fluid state cannot, by strict reasoning, be deduced from his method according to the exposition he has given of it; because the reaction of the strata on the masses on which they are laid is neglected.

Oct. 15, 1838.

JAMES IVORY.

XLIII. On a new Compound of Sulphate of Lime with Water. By JAMES F. W. JOHNSTON, F.R.S., &c. &c.*

IN the boiler of a steam-engine worked at Team Colliery near Newcastle, and fed by water from the mine, a bright steel-gray granular deposit has been several times observed, which under the microscope appears to consist of small transparent prismatic crystals discoloured by carbonaceous matter. The faces of these crystals are rounded, but according to Mr. Brooke, they are right rhombic prisms, the angles being undeterminable with any degree of accuracy.

Heated in a close vessel these crystals give off pure water, becoming opaque, and at a red heat in the air they lose their colour and become white. Heated to 220° Fahr. in the air they lose a sensible quantity of water, and kept at 240° Fahr., for a couple of hours the whole of the water is driven off, so that at a higher temperature they undergo no further loss, except what is due to the combustion of the small quantity of carbonaceous matter they contain.

Boiled in distilled water they are very sparingly soluble: the solution gives white precipitates with chloride of barium and oxalate of ammonia.

After drying at 212° , 19.424 grs. lost 1.25 = 6.435 per cent. when heated in a close vessel till all the water was driven off. The colour was still gray. Heated to redness in the air till it became white, the loss amounted to 6.728 per cent., giving 0.293 per cent. for carbonaceous matter. A second portion of 25.194 lost 1.742 when heated to redness in the air = 6.914 per cent.

Of the white salt thus heated in the air, fused with carbonate of soda, digested in distilled water, filtered, and precipitated by chloride of barium, gave

| | |
|--------------------------------|--------------------------|
| | Anhydrite by calculation |
| | contains |
| Lime | 41.734 |
| Sulphuric acid ... | 59.027 |
| The salt therefore consists of | |
| Sulphate of lime | = 93.272 |
| Water | = 6.435 |
| Carbonaceous matter | = 0.293—100. |

The formula $\text{Ca S} + \frac{1}{2} \text{H}$ gives

| | |
|------------------------|--------------|
| Sulphate of lime | = 93.843 |
| Water | = 6.157—100. |

The specific gravity of the new salt } = 2.753 and 2.761
 at 60° Fahr..... }

After heating to redness = 2.936 and 2.929

Its density, therefore, as we should expect, lies between those of gypsum and anhydrite, and after heating to redness it has precisely the specific gravity of the latter.

Density of gypsum = 2.310 Mohs, 2.322 Royer and Dumas.
 ————— new salt = 2.757.

————— anhydrite = 2.899 Mohs, 2.96 Royer and Dumas.

————— new salt } = 2.932.
 after heating }

The boiler in which this deposit is formed is worked under a pressure of nearly two atmospheres, and to this among other circumstances is probably due the formation of this singular hydrate. When sulphuric acid is added to a dilute boiling solution of chloride of calcium, crystals of gypsum with the usual quantity of water are deposited. Added hot to a solution of chloride of calcium which boiled at 265° Fahr., an earthy precipitate fell, which after drying at 212° lost by a red heat only 1.43 per cent. The temperature of a more dilute solution boiling at a heat of 220° to 230° Fahr. falls instantly to 212° Fahr., on the addition of a drop of hot sulphuric acid; the sulphate formed causing a copious evolution of vapour. I have therefore not succeeded in my attempts to form this salt artificially. The uniform high temperature maintained under a higher pressure than that of the atmosphere may be the chief cause of the formation of the salt in the steam boiler.

Is this salt a simple hydrate $2\text{Ca}\ddot{\text{S}} + \text{H}$, or is it a compound salt $\text{Ca}\ddot{\text{S}} + \text{Ca}\ddot{\text{S}}\text{H}$, or $3\text{Ca}\ddot{\text{S}} + (\text{Ca}\ddot{\text{S}} + 2\text{H})$? This question is entirely theoretical, the properties of the salt lending no direct support to either opinion. It is, however, not without interest in the present state of our knowledge in regard to the water contained in salts. The observation made in regard to the superior affinity of many sulphates for *one* in preference to the other atoms of water which their crystallized hydrates generally contain, does not apply to the sulphate of lime, and the existence of this *half hydrated* crystallized compound—if considered as a simple salt $2\text{Ca}\ddot{\text{S}} + \text{H}$ —is equally inconsistent with the idea expressed by the term *saline* water. If by this term it is intended to denote that of the 6 or 7 atoms of water with which certain sulphates combine, one of these equivalents performs a peculiar function, we have in the present salt half an atom only remaining, and this driven

off with nearly the same ease as any of the water is driven off from gypsum. It may however be a compound of anhydrite with gypsum or with the monohydrate $\text{Ca} \ddot{\text{S}}\text{H}$, in which case its constitution may be reconciled with the theoretical views of Professor Graham.

That the relations of sulphate of lime to water do not form an exception to the general laws by which those of the other sulphates belonging to the same isomorphous group are regulated, may be inferred from the discovery by Mitscherlich, of a sulphate of iron ($\text{Fe} \ddot{\text{S}} + 2\text{H}$) analogous in constitution with gypsum, and like it possessed only of a sparing solubility. This analogy leads us rather to expect other compounds of sulphate of lime with water analogous to those observed in the sulphates of iron, magnesia, &c. and that the changes produced on the analogous hydrated salts of each of the sulphates by heat and other agents should be *generally* the same.

There are strong grounds for accepting it as a general conclusion that whatever compound has been formed by one member of an isomorphic, *may* be formed, under other circumstances perhaps, by every other member of the same group. If we compare what we know with what on this principle is possible, we shall find our real knowledge to be unexpectedly small. It is something, however, to know how much in a particular line remains yet to be discovered. Even in the branch of saline compounds which has received so general an attention from chemists, and for so long a period, a comparison of this kind is calculated to astonish us by the vast number of compounds it exhibits as remaining yet to be sought for.

Let us take for example the class of hydrated sulphates to which gypsum belongs, and compare the known with the possible, as is done in the following table.

In this table 81 compounds are indicated, while of these only 18 are actually known to us, or less than one fourth part, while many more may still be possible belonging to the formula $\text{R} \ddot{\text{S}} + 4\text{H}$, and the various formulæ intermediate between seven and twelve atoms of water ($\text{R} \ddot{\text{S}} + 7\text{H}$ and $\text{R} \ddot{\text{S}} + 12\text{H}$).

The general formulæ at the heads of the columns indicate the crystalline compounds believed to be possible, the special formulæ underneath the crystalline compounds actually known.

Table exhibiting the formulæ of all the known Sulphates, hydrated and anhydrous, of the Magnesian Class of Oxides.

| Sulphates of | $\ddot{R}S$ | $\ddot{R}S + \frac{1}{2}H$ | $\ddot{R}S + H$ | — + 2H | — + 3H | — + 5H | — + 6H | — + 7H | — + 12H |
|----------------------|--------------|-----------------------------|-----------------|-------------------|------------|-------------|-------------|-------------|---|
| Lime..... | $\ddot{Ca}S$ | $\ddot{Ca}S + \frac{1}{2}H$ | | $\ddot{Ca}S + 2H$ | | | $MgS + 6H$ | $MgS + 7H$ | $MgS + 12H$ |
| Magnesia | | | | | | | $MgS + 6H$ | $MgS + 7H$ | $MgS + 12H$ |
| Protox. of Manganese | | | $MnS + H$ | | $MnS + 3H$ | $MnS + 5H$ | $MgS + 6H$ | $MgS + 7H$ | $MgS + 12H$ |
| — Iron..... | | | | $FeS + 2H$ | | | $FeS + 6H$ | $ZnS + 7H$ | $NiS + 7H$ |
| Oxide of Zinc..... | | | | | | | $ZnS + 6H$ | $ZnS + 7H$ | $NiS + 7H$ |
| — Nickel | | | | | | | $NiS + 6H$ | $NiS + 7H$ | $NiS + 7H$ |
| — Cobalt | | | | | | | $CoS + 6H$ | $CoS + 6H$ | $CoS + 6H$ |
| — Copper..... | | | $CuS + H$ | | | $CuS + 5H$ | $CuS + 5H$ | $CuS + 5H$ | $CuS + 5H$ |
| — Lead..... | PLS | | | | | | | | |
| Crystal: form | Rt. Rh. Pr. | Rt. Rh. Pr. | ? | Ob. Rh. Pr. | ? | Ob. Rh. Pr. | Ob. Rh. Pr. | Rt. Rh. Pr. | By exposing a saturated solution to 32° Fahr. (Frische p. 579.) |

* Deposited from boiling solutions: corresponding hydrates of the sulphates of zinc and magnesia may be obtained by heat, but not crystallized.

† These are obtained by heating the salts with 7 atoms of water to 40° C. in oil, or by crystallizing at high temperatures.

‡ This salt and the corresponding selenate of zinc are dimorphous, the second form being a square prism.

[¶ Views on the formation of rock-salt beds, very similar to that of Berzelius here cited, but founded, principally, on geological considerations, had previously been entertained by various geologists: a discussion of them by Mr. Brayley, in connection with inductions from certain phenomena exhibited by a saliferous formation in North America, will be found in Phil. Mag. and Annals, N.S., vol. vi. p. 71.—Edit.]

Suppose our actual knowledge of the almost countless groups of salts already partially studied to be tabulated in a similar manner, and thus compared with the unknown, how few would the cultivated spots appear, how large the water yet to be reclaimed! And whenever we consider how very few of the properties of the salts longest known have yet been determined with any degree of accuracy, how mere a skeleton does all our chemical knowledge appear! We hasten after new conquests without waiting to consolidate our dominion.

The existence and mode of formation of this salt throws some light on a point of geological interest. As anhydrite occurs only in connexion with rock-salt, it was suggested by Berzelius that the latter might be an igneous and not an aqueous deposit*. Deposited in water the sulphate of lime ought, he reasoned, to be in the state of gypsum, and not of anhydrite; but the formation of the hemihydrated salt above described, shows that the quantity of water present in such salts does not depend on the presence or absence of water only, but on the united temperature also under which the crystals are formed. Had the pressure in the boiler been so great as to raise the temperature to 260° Fahr. anhydrite would be formed in nature as it was formed on pouring sulphuric acid into a solution of chloride of calcium boiling at 265° Fahr.

Two fragments from the exterior of an apparently pure mass of anhydrite from Germany lost when heated to redness about 6 per cent. of water. The interior of the mass lost a mere trace. Does this show any tendency in anhydrite to form the new compound?

Durham, June, 1838.

XLIV. *On the Composition of certain Mineral Substances of Organic Origin.* By JAMES F. W. JOHNSTON, A.M., F.R.SS. Lond. and Ed., F.G.S., Professor of Chemistry and Mineralogy, Durham.†

VI. *Guyaquillite.*

I AM indebted to Mr. Brooke for a small quantity of a resinous substance said to form an extensive mineral deposit in the neighbourhood of Guyaquil in South America, and for which I propose the name of Guyaquillite.

Of this substance I have seen two varieties, one nearly homogeneous of a pale yellow colour, with no resinous lustre or

* See note ¶ in the preceding page. † Communicated by the Author.

fracture, not compact, but as if made up of many small portions adhering together. The other variety is mixed with a greater or less quantity of a dark brown, bituminous-like substance interposed between the small fragments of which the mass is composed.

The pure mineral is opaque, of a pale yellow colour, yields easily to the knife, may be rubbed to powder; is very slightly soluble in water, and largely in alcohol, giving yellow solutions which have an *intensely bitter taste*. This last property is highly characteristic. By slow evaporation the alcoholic solution yields pale yellow prisms. As it occurs in nature, the *Guyaquillite* has a sp. gr. of 1.092; after fusion, it may possibly be a little heavier.

It begins to melt at 157° Fahr., but continues viscid, and does not flow easily till near 212°. As it cools it adheres to the finger and exhibits much tenacity, forming fine threads when drawn out. After fusion it is semitransparent, a little darker in colour, and exhibits the resinous fracture and lustre. Heated in a close tube over a lamp, it darkens, is decomposed, and yields empyreumatic products.

It dissolves readily in a dilute solution of caustic potash and less so in caustic ammonia, giving yellow solutions, from which it is again precipitated by an acid. Sulphuric acid of commerce dissolves it in the cold, giving a dark reddish brown solution, from which water throws down the resin apparently unchanged. Muriatic acid boiled over it becomes yellow, but dissolves very little and does not appear to alter it. Strong nitric acid by the aid of heat acts upon it with the evolution of red fumes, becoming yellow. It dissolves it, however, in small quantity only, and the solution becomes milky as it cools, and deposits white flocks. It is also precipitated white by the addition of water. I have had too little of the substance to permit me further to investigate the nature of the change produced by this acid.

The action of liquid ammonia on the alcoholic solution of this substance is characteristic. The pale yellow solution, by the addition of a few drops of ammonia, gradually darkens, and ultimately becomes dark brownish red. The alcoholic solution gives a yellow precipitate, with a similar solution of acetate of lead. With one of nitrate of silver it gives at first none, but after standing for several hours a small quantity of a very dark precipitate shows itself. The addition of ammonia determines a brown precipitate, which speedily darkens and assumes a deep purple or black colour. This mineral substance, therefore, like the resin of retin asphalt (retinic acid), belongs to the class of acid resins.

Burned with oxide of copper from which the moisture had been well pumped out,

7.538 grs. gave 20.9 grs. of carbonic acid and 5.545 of water.

8.415 grs. — 23.54 grs. ————— 6.208 ———

These are equivalent to

| | I. | II. |
|--------------|----------|--------|
| Carbon | = 76.665 | 77.350 |
| Hydrogen... | = 8.174 | 8.197 |
| Oxygen..... | = 15.161 | 14.453 |

100.

100.

The formula $C_{20}H_{13}O_3$ gives the proportions

$$20\text{ C} = 1528.750 = 76.783$$

$$13\text{ H} = 162.2348 = 8.148$$

$$3\text{ O} = 300.000 = 15.069$$

1990.9848

100.

It is difficult to determine what is the rational formula of this compound. It may either be an oxide

or a hydrate $\begin{matrix} C_{20}H_{13} + O_3 \\ C_{20}H_{10} + 3H_2O \end{matrix}$,

or a changed oil of turpentine $C_{20}H_{16} \left\{ \begin{matrix} -3H \\ +3O \end{matrix} \right.$ in which three equivalents of hydrogen are replaced by three of oxygen.

The ratio of the carbon to the hydrogen, 20 : 13, renders it unlikely that it is a simple oxide; and that it is not a hydrate is rendered very probable by the fact that the ratio of these two elements remains the same in the guyaquillate of silver.

Thus a portion of a silver salt of a dark colour gave on burning with oxide of copper,

$$\text{Carbon.....} = 17.675 = 20 \text{ atoms.}$$

$$\text{Hydrogen ...} = 4.754 = 13.22 \text{ atoms.}$$

The third formula therefore, $C_{20}H_{16} \left\{ \begin{matrix} -3H \\ +3O \end{matrix} \right.$ or simply

$C_{20}H_{13}O_3$, gives the most probable representation of the true constitution of this substance. Of course the relation to oil of turpentine indicated by the formula, though interesting and agreeing with the doctrine of substitutions, must be viewed with distrust, till the geological position and probable origin of the mineral be more fully investigated. It is easy to suppose it, like amber, of vegetable origin, and to occur in consi-

derable quantities on the site of ancient forests of resiniferous trees; in which case the connexion with oil of turpentine would be at once beautiful and easily understood; but if it occur, as is said to be the case, in extensive layers, its immediate source is not so easily understood. Were it volatile without decomposition we could also account for its sublimation like sulphur, or its distillation like petroleum by the agency of the volcanic heat so extensively diffused beneath the South American continent; but as the affinity of its elements appears to be overcome by a comparatively moderate temperature, we must still remain in doubt as to whether in its present state it is to be considered as more immediately or more remotely of vegetable origin. At the same time it is worthy of remark, that this is not the only instance of a true resin being *said to occur* in large quantity as a mineral product in South America. I have in my possession a specimen of a peculiar dark-coloured resin, which, as my friend Mr. Fryer of Whitley-house, informs me, is found in a species of pitch lake in the desert of St. Juan de Barengela, and is thence transported in large quantities to the coast for the use of the shipping. Of this substance I shall give a description and analysis in a future communication.

Guyaquillate of Silver.—To determine the atomic weight of this resin, I prepared several portions of the salt of silver. It appears, however, to form, in common with most of the acid resins, both neutral or acid and basic salts, as I have been unable in the several trials which the quantity of the substance at my disposal permitted me to make, to obtain two portions of precisely the same constitution. This may also have arisen partly from the necessity I have been under of using the less pure variety for solution, as my supply of the unmixed was exhausted. The mixture however consists in great part of a bituminous matter insoluble in alcohol.

1. Of a portion which had subsided gradually on the addition of ammonia and was perfectly black, 6·719 grs. left 4·728 grs. of metallic silver = 75·573 per cent. of oxide.

2. A portion of a second preparation gave only 68·861 of oxide; but this was evidently a mixture, for when boiled again in alcohol and collected on the filter it gave 72·266 per cent. of oxide of silver. These two results indicate an equivalent = $\frac{1}{4}$ ($C_{20} H_{13} O_3$), since a compound represented by $\frac{1}{4}$ ($C_{20} H_{13} O_3$) + Ag O would contain 74·512 per cent. of oxide.

3. The hot alcohol in which No. 2 was boiled, deposited on cooling a brown precipitate, which gave on burning only 14·939 per cent. of oxide of silver.

4 ($C_{20}H_{13}O_3$) + Ag O contains 14.417 per cent. of oxide.

4. A third portion separately prepared, which was brown when precipitated, but had become black, gave 26.888 of oxide of silver.

2 ($C_{20}H_{13}O_3$) + AgO contains 26.715 of oxide of silver.

We may conclude therefore that this acid forms different classes of salts with the same base, which possess different degrees of solubility; the conditions necessary to ensure the constant production of the same compound, and the determination of which are the neutral and which the acid or basic salts, will require further investigation.

Durham, June, 1838.

XLV. On the primary Forces of Electricity. By

RICHARD LAMING, Esq., M.R.C.S.*

[Continued from p. 54.]

Part II.

66. **I**T may be useful before we proceed to consider the origin and propagation of induction among the atoms of contiguous bodies, to recapitulate some of the chief particulars in the new theory which are thought to be established as facts in the preceding part of this paper.

1st. The attraction which is reciprocal between electricity and common matter is definite with regard to quantity as well as force; no kind of matter ever attracting either more or less of electricity than the quantity which constitutes its natural equivalent (3.).

2nd. Besides the above force, designated the major electrical attraction, there is a second by which the atoms of electricity are associated together; and this we have called the minor electrical attraction (15.).

3rd. The minor electrical attraction is the alone cause of bodies becoming plus or positively electrical (16.).

4th. Electrical induction is immediately dependent on the definite nature of the major electrical attraction (7.).

5th. The electrical condition of a body under induction virtually or really compensates the opposite electrical state of the inducing body (11.).

6th. Induction, and consequently compensation, in all cases precede the sensible locomotion of matter caused by the action of the major electrical attraction; whether it be

* Communicated by the Author.—*Erratum*, p. 45. Art. 34, eighth line, for "as the densities of the air directly," read "as the densities of the air inversely."

that of free electricity moving towards its minus compensator, or of common matter, either plus or minus, moving towards other common matter in the converse electrical condition.

7th. Electrical discharges are always more or less retarded by the action of the minor electrical force in the plus body (43.).

8th. Free electricity may be conducted by the inductive influence of the major force from plus to minus bodies (54. 55. 60. 61.).

67. In addition to these several facts it was shown that the law of Coulomb is the law of the major electrical force; and by necessary inference of electrical induction also. Hence it follows that no mass of common matter can be dispossessed of the least portion of its natural electricity by the major force of free electricity acting on it at a sensible distance; and thus we learn at once that induction can only be established on contiguous atoms.

The manner in which induction is originated will almost immediately be brought under consideration; but we purpose, on the assumption of its existence, to trace first the mode of its propagation among contiguous atoms.

68. We have seen that whenever by virtue of the minor force free electricity is attached to an insulated body, that body must be surrounded by an insulating medium either actually or virtually in a minus electrical condition (32.); if we conceive the plus body to be a sphere freely insulated in the atmosphere, the spherical stratum of aërial atoms immediately in contact with it will, in order to compensate its charge, *virtually* dismiss portions of their own natural quantities; but the dismissal being only virtual and not real, this first stratum will become plus also (12.); a second spherical stratum of aërial atoms, by compensating the former, will in their turn become plus; and these in like manner impress a similar condition on a third; the third on a fourth, and so on *ad infinitum*, or until the action be terminated by the presence of some uninsulated body capable of *actually* dismissing the requisite portion of its natural electricity.

69. Now if the uninsulated and really minus compensator be free to move, its common matter, being attracted by the virtually plus air compensated by it, will receive an impulse in the direction of the charged sphere. After moving a little, the uninsulated body will assume the compensation of, and therefore be attracted by, the inner portion of air next contiguous; then moving again it will approach a third, and

other successive portions of air, until it is brought to rest against the charged sphere itself, by which the whole line of inductive action was occasioned.

70. As it is with visible attraction, so also is it in the case of electrical discharges; the free electricity is attracted onward towards the uninsulated compensator by each of the aerial strata in succession, until at length attaching itself to the former it returns to a state of natural equilibrium.

71. These principles enable us to understand that neither the discharge of free electricity which we may observe to take place from plus to minus bodies, nor the reciprocal attraction of such bodies by one another, is affected by any direct action they might be supposed to have on one another, but, on the contrary, through the medium of intervening matter; consequently it is only in a restricted sense that we may speak of distant bodies compensating or attracting one another. Hence the law of Coulomb must be received only as conventional, for although it embraces phenomena with accuracy, it is palpably fallacious in theory.

72. We have it in our power, by a very simple method, to verify the conclusion respecting the contiguous action of induction, to which we have thus been conducted by the theory. It is in principle as follows: let a plus body be separated from its compensator by an insulating medium of determinate thickness, and the electrical condition induced in the compensator be estimated in the ordinary manner by an electrometer. Then, if induction be independent of the insulating medium, the merging of a second equally plus body, and also of a second similar compensator, in the first respectively should add nothing to the intensity of the induced charge; or to reduce the case to practice, the capacity of a Leyden jar should increase with the thickness of its metallic coatings, minus some little on account of the minute increase in their mean distances. Now I find, and I believe the fact to be sufficiently notorious, that a given electrical charge on one of the coatings of a Leyden pane will exhibit precisely the same intensity whether the coatings be composed of films of Dutch metal only, or the very much thicker leaf of tinfoil, even though it be several times repeated.

73. The view of induction now presented opens up to us the *cause* of Coulomb's law; thus advancing us in knowledge another step towards that incomprehensible link which connects the immaterial impulse of the Divine Will with the material creation. To understand this cause let us imagine a quantity of free electricity to be accumulated in an insulated

point and surrounded by concentric strata of a compensating atmosphere (68.); then, the strata being all of equal thickness, either will contain a number of electrical atoms varying directly as the square of its distance from the centre. Now, assuming, what is strictly agreeable to sound reasoning, that the induction on any stratum is equally distributed among all the atoms contained in it, and knowing from the definite nature of the major electrical force that the sum total of the inductive force exerted by the charge is a constant quantity in each of the strata, it must of course follow that the force upon any given atom, in either of the strata, is as the square of its particular distance from the centre inversely.

74. Having thus shown that induction is extended to bodies at sensible distances by the intervention of others in immediate contiguity, we may proceed to investigate the manner of its origin; and for this purpose it is requisite to trace more minutely than we have yet had occasion to do, the connection of the electrical with common atoms.

75. It will doubtless be admitted that if the principles we have hitherto advocated be in the general consistent with facts, each atom of common matter is enveloped in an atmosphere of electrical atoms; or as we shall find it convenient to express it, each common atom constitutes a centre of major electrical force to an electrosphere. Now without entering here into the question of the relative volumes of these electrospheres, it will be sufficient for our present purpose to examine the manner in which they generally are connected with common atoms, and the reciprocal action under particular circumstances of such as are of equal volume.

76. In the first place it may be premised, that we have no reason for asserting that the major force which is reciprocal between a common atom and the electrical atoms which form its equivalent electrosphere, observes the law of Coulomb: for the inductive action on which that law depends obviously commences at the surface of the electrospheres; on the other hand it will be admitted that this fact supplies no argument against such a supposition; and at all events we are quite sure, whatever be the ratio, that those atoms of an electrosphere which are nearer to the central nucleus are attracted with the greater forces; for were it not so the retarding forces to the compensation of successive increments of free electricity to a plus body could not progressively increase as we have already proved them to do (6.).

77. This being understood we may proceed at once to our task. Let us conceive the electrical equivalent of an atom of

common matter to surround it in the form of any number of concentric strata; the major force of the central nucleus upon all the electrical atoms will then vary in some unknown ratio inversely as their distances respectively. Hence an atom of free electricity attracted to the surface of such an electrosphere by the minor force, being necessarily the most distant atom, and, so far as the equivalent of the common atom is concerned, supernumerary, could never become attracted by that common atom; under such circumstances, therefore, the free atom would exercise no inductive action whatever to cause any portion of the contents of the electrosphere to become liberated.

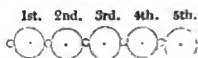
78. Now let us suppose the preceding case to be repeated, with this only modification, namely, that the outer stratum of the electrosphere by which the electrical equivalent of the common atom is completed were deficient some one or more of the number of electrical atoms requisite to constitute it a perfect shell; and we shall find the result to be very different. To fix and simplify our ideas, let us imagine the electrical equivalent to be completed by the existence of a single electrical atom on the surface of the last perfect stratum of the electrosphere, and an atom of free electricity to be artificially placed against that surface; in such a case two electrical atoms would obviously be at the same distance from the centre, and consequently the more proximate hemisphere of each engage the major force of the common nucleus.

79. In the next place let there be a second *similar* electrosphere in contact with the former one; the tendency of the two exterior electrical atoms, as also that of the added free atom, under the influence of the *minor* force, will be to place them all in a triangular position in a plane separating the two electrospheres. Whether in this or any other situation, *two* of them will be equally held by the major force of the two common nuclei, each of the latter acting on proximate hemispheres; the third atom of electricity being retained in connection simply by the minor force.

80. But as in electrical investigations we experiment not with atoms but with masses, we have to modify this view of the positions of the exterior atoms of the electrical equivalent. If a homogeneous body be composed of electrospheres similar to those last imagined (78. 79.), the exterior electrical atoms of the electrospheres, considered generally, will be equidistant, or nearly so, from one another, being equally attracted in opposite directions by the minor force in contiguous electro-

spheres *; consequently the natural position of a line of such electrospheres will be as shown in the annexed figure, in which the points represent the nuclei of common matter; the larger circles the boundaries of the perfect strata of the electrospheres; and the smaller circles the exterior atoms of electricity by which the equivalents of the common nuclei are completed.

Fig. 3.



81. Now, on presenting to the first of such a line of electrospheres any body in a plus electrical condition, it is quite manifest by the principles we are examining, that the major force will induce the exterior atom of the first electrical equivalent to take the place of the second, this of a third, and so on, to the end of the line where the exterior atom will appear free; or otherwise it will be found at that place in the series where it can obtain the most perfect compensation, and beyond which of course the inductive process would not be continued.

82. If we conceive the last electrosphere in the line to be so circumstanced, with regard to contiguous bodies that it can give off its exterior and free atom as rapidly as it becomes replaced, the inductive action just described may be repeated an indefinite number of times, or so long as the plus electrical body at the other extremity retains a single free atom to complete the equivalent of the proximate electrosphere. In this case the whole line performs the function of a conductor of electricity.

83. Inasmuch as induction and compensation are always simultaneous they may in a certain sense be regarded as synonymous terms; but for sake of theoretical precision it will be useful not only to regard the latter as the *effect* of the former, but also to subdivide compensation into *virtual* and *actual*. By virtual compensation we should understand that state of a given body, in which, though exactly retaining its natural electrical equivalent, it exhibits under the influence of induction the characteristics of a charged body; and by actual compensation we might describe that other condition in which a body is placed by induction when it really possesses a quantity of electricity different to its natural equivalent.

84. If our explanation of the origin of the inductive pro-

* The agency by which electricity is made elastic is here assumed to be in operation; its nature will be the subject of a future communication.

cess be expressive of the truth, we shall perceive, first, that the susceptibility to induction in electrospheres will increase with the number of electrical atoms in their external and imperfect strata, so long as the number is not too great for all of them to be equidistant from the two contiguous central nuclei; and, secondly, as the limited number will be greater as the electrospheres are more voluminous, that the inductive susceptibility will be the more exalted as the electrospheres are larger. Now we have found reason to believe that induction, as evidenced by compensation, increases with the atomic weights of bodies (28.); and if this be true, the conclusion to be deduced from the fact obviously is, that the volumes of electrospheres vary as their weights, or in other words, that the minor electrical force is the cause of gravitation.

85. The justness of such a conclusion will be made more apparent in the next part of this treatise; in which we shall endeavour to show that the principles we have promulgated are adequate to explain all the phenomena of electrical excitation.

[To be continued]

XLVI. *The Specific Heats of the Gases as deduced by Dr. Apjohn, compared with the more recent Results of Dr. Suerman.* By JAMES APJOHN, M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Ireland.

[Continued from p. 273, and concluded.]

AT the close of the paper which I have submitted to the Royal Irish Academy, I have hazarded the following propositions, which would seem to be justified by the results of my researches.

1. The simple law so much insisted upon in modern times by Haycraft, Marcet, and De la Rive, and others, that equal volumes of the different gases have the same specific heat, is not the law of nature.

2. The more limited conclusion announced by Dulong, that the *simple* gases have under equal volumes the same specific heat, is probably not true in a single instance, and is altogether at variance with my result for hydrogen.

3. The numbers at which I have arrived correspond tolerably well with those of De la Roche and Berard except in the case of hydrogen, to which I ascribe a specific heat greater

than that assigned to it by these philosophers in the ratio very nearly of 5 to 3.

4. There does not seem to be any simple relation between the specific heats of the gases and their specific gravities, or atomic weights, and philosophers in searching for such are probably pursuing a chimera.

From this exposition of my method, and summary of my experiments, I now turn to the more recent researches of Suerman. These researches have appeared in the form of an inaugural essay, which was published by the author upon the occasion of his taking a degree in the university of Utrecht. He had, he informs us, for a long time determined upon making the specific heats of the gases the subject of his thesis; but when after many interruptions he had at length begun to provide himself with the necessary apparatus, he was much chagrined at finding, upon looking into a recent number of the Philosophical Magazine, that I had already investigated the same question, and by the very method which it was his intention to have employed. He would, in fact, have turned to something else were it not for the persuasions of Professor Moll, who urged him strongly to persevere in his undertaking. This he was fortunately induced to do, and the result has been a most elaborate and lucid history of the various attempts which have been made towards the solution of this very difficult problem in physics, followed by a detailed account of a number of experiments instituted by himself with a similar object.

His method I have stated to be the same with that which I had previously adopted. This is strictly true. The formula, however, which he used in calculating his observations, and which he stated to be due to Gay-Lussac, is slightly different from that which I have employed. In part retaining the notation already used, and slightly changing the form of his expression, it will become

$$a = \frac{f'e}{48d} \times \frac{30}{p-f'},$$

an equation differing in no respect from formula (C) which I have used, save in the substitution of $p-f'$ for p I do not of course admit the correctness of this expression; $p-f'$ is certainly the elasticity of the gas when saturated with vapour at temperature t' . But it is the pressure of the *dry* gas which should enter into the formula, and this is p , provided p represents the height of the barometer at the time of the experiment. If this be a true view of the question, it is clear that Suerman's results are all a little too high, and should, in

order to their being comparable with mine, be multiplied by $\frac{p-f'}{p}$.

The apparatus employed by Suerman was one of a very ingenious but rather complicated description, and, as an exact idea of all its parts cannot be well conveyed without a diagram, for a detailed account of it I must refer to his own essay. The general principle, however, on which it acted is easily explained. The air or gas which was the subject of experiment was, under the pressure of a column of water which was maintained constant by the well-known contrivance of Mariotte, forced from a gasometer through oil of vitriol contained in an adjacent Wolfe's bottle, and thence through a tube packed with fragments of fused chloride of calcium. Escaping from the tube in a state of desiccation, it was next made to enter one of sheet tin, through the sides of which were inserted a manometer, to show the interior pressure, and a wet and dry thermometer; and having traversed this it passed on to a second gasometer, from whence it was made by the same means, if necessary, to return to its original reservoir, flowing in succession through the oil of vitriol, the chloride of calcium, and the calorimeter or tube containing the manometer and thermometers. In the experiments on atmospheric air, the pressure of water was employed; but the other gases, lest they should become contaminated by the air with which water is naturally impregnated, were impelled through the apparatus by the hydrostatic pressure of a saturated solution of common salt, which was selected on account of the very small amount of air which it is capable of absorbing. From the sketch given of the apparatus (p. 270) the reader will have perceived that when the air or gas has been forced from gasometer A into gasometer B, it is possible to continue the current with scarcely any interruption, and cause it to return from B to A, passing of course in both cases in a state of perfect dryness through the tube containing the thermometers. It being, however, difficult to render the second current perfectly continuous, and identical in velocity with the first, Suerman completed his experiment in every instance while the elastic fluid was passing from A to B, by resorting to the simple expedient of cooling the wet thermometer before its introduction into the apparatus, by means of which the stationary temperature was attained long before the contents of gasometer A had been expelled. To this description it is only necessary to add, that, by the proper management of a stop-cock placed at the extremity of the tube containing the thermometers, he was enabled so to regulate the blast during each

experiment that the fluid stood at the same level in the two arms of the manometer, and that, therefore, the value of p was given by the height of the barometer.

The following are, in a tabular form, some of his observations, the pressure being converted from centimetres into inches, and the temperatures reduced from the centigrade scale to that of Fahrenheit. The number of his experiments with air was 12, with oxygen 9, with hydrogen 9, with carbonic oxide 6, with nitrous oxide 6, and carbonic acid 8. But two for each gas are given here, namely, that in which t was a maximum, and that in which it was a minimum, for the several results in each series are so consistent that it would answer no useful purpose to quote them all.

| | | t | t' | d | p | Sp. Heats under equal Weights.
(Suerman, J. A.) | | |
|----------------|----|-------|-------|-------|--------|--|--------|--------|
| | | | | | | | | |
| Atmosph. Air | 1. | 74.97 | 47.30 | 27.17 | 30.636 | .3033 | .3024 | .2945 |
| | 2. | 64.27 | 41.45 | 22.78 | 29.572 | .3015 | | |
| Oxygen | 1. | 66.48 | 42.74 | 23.74 | 29.811 | .2724 | .2739 | .2670 |
| | 2. | 65.52 | 42.57 | 22.95 | 30.016 | .2754 | | |
| Hydrogen | 1. | 64.85 | 46.11 | 18.74 | 29.619 | 6.2490 | 6.2094 | 6.0189 |
| | 2. | 63.16 | 45.05 | 18.11 | 29.813 | 6.1698 | | |
| Carbonic Oxide | 1. | 73.62 | 46.68 | 26.94 | 29.991 | .3148 | .3142 | .3004 |
| | 2. | 70.02 | 45.16 | 24.86 | 30.579 | .3137 | | |
| Nitrous Oxide | 1. | 70.25 | 46.11 | 24.14 | 30.017 | .2156 | .2184 | .2132 |
| | 2. | 69.12 | 45.95 | 23.17 | 30.291 | .2212 | | |
| Carbonic Acid | 1. | 69.01 | 45.05 | 23.96 | 29.670 | .2112 | .2153 | .2087 |
| | 2. | 66.65 | 44.37 | 22.28 | 30.170 | .2194 | | |

The numbers in the last column of preceding table, are those which result from the formula $a = \frac{f' e}{48 d s} \times \frac{30}{p}$, which I have applied in my own researches, and they are all, it will be seen, less than the corresponding numbers in the preceding column, or those deduced by Suerman himself.

The following table exhibits in adjacent columns the specific heats of the respective gases under *equal weights* and *equal volumes* as given by Suerman, and as deduced from his experiments by my formula, the specific heat of air being in both cases represented by unity.

| | Sp. Heats of equal Weights. | | Sp. Heats of equal Volumes. | |
|-------------------|-----------------------------|---------|-----------------------------|--------|
| | Suerman. | J. A. | Suerman. | J. A. |
| Air..... | 1.0000 | 1.0000 | 1.0000 | 1.0000 |
| Oxygen | .9057 | .9066 | .9986 | 1.0000 |
| Hydrogen | 20.5337 | 20.4376 | 1.4126 | 1.4061 |
| Carbonic Oxide | 1.0390 | 1.0200 | .9722 | .9921 |
| Nitrous Oxide ... | .7222 | .7239 | 1.1005 | 1.1035 |
| Carbonic Acid ... | .7106 | .7086 | 1.0853 | 1.0808 |

The accordance is here sufficient to justify the conclusion that the specific heats, as far at least as respects their relative values, are not materially affected by substituting my formula for that of Gay-Lussac employed by Suerman. We may therefore place, for the purpose of comparison, his mean results in juxtaposition with those at which I have arrived.

| | Sp. Heats of equal Volumes. | | Sp. Heats of equal Weights. Air = 1. | |
|-----------------|-----------------------------|--------|--|---------|
| | Suerman. | J. A. | Suerman. | J. A. |
| Atmospheric Air | 1·0000 | 1·0000 | 1·0000 | 1·0000 |
| Oxygen | ·9954 | ·8080 | ·9028 | ·7328 |
| Hydrogen | 1·3979 | 1·4590 | 20·3191 | 21·2064 |
| Carbonic Oxide | ·9923 | ·9960 | 1·0253 | 1·0239 |
| Nitrous Oxide | 1·1229 | 1·1930 | ·7354 | ·7827 |
| Carbonic Acid | 1·0655 | 1·1950 | ·6975 | ·7838 |

A glance at this table is sufficient to show that, (oxygen being excepted) the numbers of Suerman and mine are almost identical. His number indeed for carbonic acid is somewhat less than mine, but the difference is quite within the limits of the probable errors of observation in such experiments. And here I may observe that M. Suerman has given an erroneous statement of my results, for which, however, I am myself to blame. During the meeting of the British Association for the Advancement of Science in Dublin, I laid before the chemical section some experiments which I had just concluded, in reference to the subject under consideration, a brief account of which was published in the succeeding number of the reports of the Association*. The numbers given there are set down as the specific heats under equal weights, whereas the division by the specific gravities having accidentally been omitted, they were in reality the specific heats of equal volumes.

M. Suerman was aware of this omission, and has applied the necessary correction. The numbers, however, at which he has thus arrived do not still correctly represent my experiments, and for the following reason. The gases operated upon by me having all contained some atmospherical air, a correction, as has been already explained, had to be applied to the specific heats obtained from my hygrometric expression. Now the formula used for this purpose affected the correction upon the supposition of the numbers to which it was applied being the specific heats of equal weights, whereas they were in point of fact the specific heats of equal volumes. This cir-

[* See Lond. and Edinb. Phil. Mag., vol. vii. p. 385.]

cumstance has been overlooked by M. Suerman, and he has as a consequence deduced from my experiments numbers for the specific heats of the different gases materially higher than the true. This is the chief cause of the difference between his numbers and mine as they appear in his thesis 6·89; but the difference in question is also partly attributable to the circumstance of his having had access only to my first series of experiments.

The numbers then which I have obtained are 8·6, the same with Suerman's. This is true of them relatively but not absolutely, for his values of a for air and the different gases are in every instance greater than mine. This is partly owing, as has been already explained, to the use of the division $p-f'$ instead of p in his formula, and to his assigning to e a higher value than I have given it. The chief cause, however, is that in his experiments the depressions were in no instance as great as those which I obtained; and this brings me to observe upon what I consider as a great defect in the apparatus which he employed. The tube containing the thermometers was, I conceive, too large, and its shape badly chosen, so that from both circumstances combined, it was scarcely possible that the air brought once in contact with the moistened bulb should be with certainty immediately displaced. The current in fact, which swept by the thermometers, being intermixed with gas already in the tube in a state of comparative stagnation, and which gas had necessarily acquired moisture from the wet thermometer, the depression indicated by the latter instrument was not that due to the elastic fluid in a state of perfect desiccation, but the feebler one caused by gas already charged with a certain amount of humidity. It is very true, as Mr. Suerman remarks, p. 23, that the diameter of the tube containing the thermometers cannot be reduced beyond a certain point, for in such case, unless indeed the rapidity of the current be greatly augmented, the effect of the radiation of the sides of the tube upon the thermometer would become very sensible, and cause the instrument prematurely to reach its stationary condition. My objection, however, is not so much to the absolute as to the relative magnitude of the tube in question. It was larger than that of the tubes which convey the gas into and from it, and hence the permanent contact of a certain quantity of moist air with the bulb of the wet thermometer was inevitable. In my experiments the two thermometers were placed lengthwise in a glass tube whose calibre was somewhat less than that of the other passages of the apparatus, so that the column of gas which it included was necessarily altogether successively

thrust forward, and the wet bulb thus permanently subjected to the action of a gaseous current in a state of perfect dryness.

Having stated this, which I consider as the gravest objection to which the experiments of Suerman are exposed, I shall next advert to, for the purpose of correcting, a couple of mistakes into which he has fallen in reference to my researches upon the subject of specific heats.

In the first place then Suerman, p. 81, seems to think that in my computations I have considered the caloric of the elasticity of vapour as an invariable quantity, whereas in point of fact it is the sum of it and the sensible heat of vapour whose value theory and experiment would seem to concur in proving to be constant. This is a misapprehension. In my first paper on the dew-point, p. 4, will be found the following sentence: "In strictness the number employed (to represent e , the latent heat of vapour) should be $967 + 212 - t$, but it would be easy to show that the uniform use of 1129 (the value of e at 50°) cannot give rise to any material error." The latter part of this sentence was intended to apply solely to the meteorological use of my formula, and not at all to it when employed in investigating the question of gaseous specific heat. Dr. Suerman has fallen into this misconception from the circumstance of his having seen only the abridged account of my first series of researches on this subject as published in the Reports of the British Association for the Promotion of Science. Had he however repeated the calculation of any one of my experiments, he would have seen that in the case of the specific heats I had adopted what he considers as the rigorous method of estimating the value of e , although I set down the sum of the sensible and latent heat of steam at every temperature as somewhat less than was done by him.

In reference to the second point, it is only necessary for me to recall to the attention of the reader the explanatory statement which I have already made.

From the last table given above it will be seen that, if we except oxygen, Suerman's results and mine are almost coincident. This correspondence has been noticed and admitted by him, as is obvious from the following passage: "*Siquidem ad diversissimum attendamus apparatus quo usus est, fatendum: satis bene illis convenire experimenta D. Apjohn atque nostra. Utraque vero multum distant ab experimentis De la Roche atque Berard, ad quæ nostra propius accedunt, ratione fluidorum elasticorum elementariorum, experimenta D. Apjohn ratione aerum compositorum.*" The opinion here expressed cannot be considered as very well sustained by the

comparative view of our results which he gives at p. 89. There is in fact a material difference between his numbers and those which he ascribes to me; for to confine ourselves to a single example, the specific heat of hydrogen compared to that of an equal volume of air as determined by him is 1.3979, and, by me (as he alleges) 1.8948. But if the reader will refer to my first series of experiments, which alone were seen by Suerman previous to the publication of his Thesis, he will find the result for hydrogen to be 1.506, or but very little higher than that arrived at by the Dutch philosopher. The number therefore 1.8948 is not at all a consequence of my experiments; but it was nevertheless, as has been already fully explained, very natural that it should have been so considered by those who had merely seen the *results* of my researches.

The full explanation of this point has been already given, so that it is sufficient for me to observe here that Suerman was ignorant of it, and considering the numbers first published by me as correctly representing my experiments, was not enabled to see that the general correspondence which he recognised between the results of our investigations amounted to an almost perfect identity.

Having set myself right with the reader on these points, I have next to draw attention to some observations of Suerman's of great interest and importance in relation to the principle of the process which both he and I have adopted in investigating the question of the specific heats of the gases. In some preliminary experiments performed by him, in which a comparatively feeble pressure was employed, the experimental depressions were much less than those which his formula (that of Gay-Lussac) led him to expect. He therefore augmented the pressure, with the view of augmenting the rapidity of the current, and found that the experimental value of $t-t'$ was thus increased. What is the cause of this? and is there any reason to believe that it is in our power to give such a degree of velocity to the current as to conduct in every instance not only to the maximum difference between the indications of the wet and dry thermometers, but to such difference as may be considered to be the accurate measure of the specific heat or quantity which it is sought to investigate?

The cause of the superior influence of the augmented current is correctly assigned by Suerman. The caloric of elasticity of the vapour derived from the water of the wet thermometer comes chiefly from the air which impinges upon the moistened bulb, but partly also by radiation from the sides of the tube in which the instruments are placed. Now the caloric

derived from the former source necessarily augments with the rapidity of the gaseous current, and in a somewhat quicker ratio, owing to the increased depression, while the heat derived from radiation is in its amount uninfluenced by the speed of the blast save in as far as such speed affects the difference between the stationary temperatures of the wet and dry thermometers. The relative amount therefore of the caloric obtained from the former source will obviously rise with the velocity of the current of air or gas; and if this velocity be rendered very great, it is easy to conceive that the caloric of radiation may be a negligible quantity, and that the latent heat of the vapour formed may be considered without sensible error as exclusively derived from the aeriform fluid which is the subject of experiment. In this latter case the depression is a maximum, and with a quick blast it is greater than with a slow one, because in such case, the caloric of the vapour being derived in greater relative quantity from the gas, this latter must be cooled through a greater number of degrees in order that the heat which it evolves should be larger in amount. So far the matter is sufficiently plain, and indeed presents no difficulty whatever. Doubts, however, may be entertained, whether, with any velocity of blast which can be conveniently employed, the radiation of the sides of the tube can be prevented from exerting an appreciable influence; and whether, with a given velocity of blast, the reduction of the depression due to radiation may not be different in the case of different gases.

In reference to the first of these two points we are without any precise information. It is certain, as we have seen, that radiation must always tend to diminish the value of $t - t'$, but the degree of its influence remains to be ascertained. In relation to the second point M. Suerman has hazarded some opinions, in the correctness of which I certainly cannot concur. It is well known that gases differ materially as to the mobility of their particles, and that this mobility follows some ratio reciprocal to that of the specific gravity, so that if the same heat be applied to equal volumes of hydrogen and carbonic acid, it is propagated through the former with much greater velocity than through the latter. This, as is well known, is one of the causes which renders the method of investigating the specific heats of gases by the *velocity of heating* incapable of yielding accurate results. Now M. Suerman expresses a strong opinion that this difference of mobility between the integrant molecules of different gases has an influence also upon the cold of evaporation. "*Vix autem dubitandum videtur quin et frigus evaporationis aliquatenus hinc pendeat.*" This is to me quite unintelligible. How can the difference

of mobility in question have any influence if the gases be all made to move over the thermometers with the same velocity? If indeed a wet thermometer were to be immersed in atmospheres of the different gases either absolutely dry, or in the same hygrometrical state, the depression might be expected to be greatest in those whose molecules were most mobile. But it appears to me very obvious that in the methods of experimenting adopted by M. Suerman, as all the gases, after contact with the wet thermometer, are by the mechanical means employed removed with the same degree of speed, the reduction of temperature experienced by this instrument must in every instance be altogether independent of any peculiarity in the constitution of the respective gases, in virtue of which heat may be propagated through some with greater facility than through others. But it is not necessary to insist further on this point, for M. Suerman materially modifies the opinion previously expressed by him, as will be seen by the following extract from page 90 of his Thesis: "*Corrigitur quidem pro maximâ parte hic effectus auctâ motus celeritate, num vero omnino ita tollatur, satis certò affirmari non potest.*"

But there is another position of M. Suerman's which appears to me more untenable still. In his experiments all the gases were driven over the thermometers with the same velocity, so that the quantities by weight which passed them in a given time were necessarily proportional to their specific gravities. Now in reference to this circumstance Suerman has the following passage: "*Relativus autem radiationis ex cylindro continenti effectus hinc pendere debet. Qui, ut idem esset, celeritas motus in diversis fluidis elasticis ita fuisset modificanda, ut, cæteris paribus, rationem inversam servaret ponderum specificorum.*" Here an influence is ascribed to mass which I cannot comprehend, for it is asserted that the relative effect of radiation is not the same for all the gases, unless the same ponderable amount of each passes in a given time over the wet thermometer. The very opposite of this proposition appears to me to be the truth. We have already seen that the ratio borne by the caloric which radiates from the sides of the tube to that extricated from the air cooled by contact with the moistened bulb, diminishes as the velocity of the current increases. Surely then, in order that this ratio be the same for the different gases, they must be all made to move with the same degree of speed. I do not, in fact, see what we have at all to do with the consideration of mass, and its introduction appears to me only calculated to confuse and mislead.

In my experiments, though the same degree of pressure was applied to the different gases, they were driven from the one gasometer to the other at very different rates, hydrogen much quicker than nitrogen, air, or carbonic oxide, and these again quicker than nitrous oxide or carbonic acid. Notwithstanding, however, this difference of velocity, my results, as has been shown, are relatively the same as those of Suerman, from which we are entitled to infer that, provided the rate according to which the gases rush by the wet thermometer does not fall below a particular point, a variation of it does not sensibly affect the observed depressions.

But it is time to terminate these remarks, and I shall do so with the following series of propositions:

1. The results of Suerman are relatively the same as those at which I have arrived.

2. His *absolute* specific heats are greater than mine, partly because of a difference between our formulæ, and his attributing to the latent heat of aqueous vapour a higher value than I have done, but chiefly because the depressions obtained by him were uniformly less than mine.

3. His apparatus though very ingenious was complex, and in consequence of the dimensions and shape of the tube containing the thermometers could not give the maximum value of $t-t'$. My apparatus was simple, very easily operated with, and as it gave greater depressions must have yielded results closer to the truth.

4. His apparatus was more perfect than mine in the circumstance of its having attached to it a manometer, by which he was enabled to ascertain the actual pressure of the current of air or gas. In mine there was no such provision, but the augmentation of pressure was in every instance the same, and must also, from the manner in which the gasometers were connected, have been necessarily very small in amount.

5. All results obtained by the method which we have employed must, theoretically speaking, be higher than the true, as in consequence of the heat radiated from the sides of the containing tube upon the wet thermometer, it is prevented from reaching its extreme point of depression.

6. Our researches completely refute the idea of the gases, whether simple or compound, having under equal volumes equal specific heats, and establish the very singular fact of hydrogen having a specific heat very nearly once and a half as high as that usually attributed to it.

P.S. I should not omit to mention that Suerman has investigated by the same method, and by means of a very simple and admirable apparatus, the specific heat of atmospheric air

at pressures less than those of the atmosphere. Eighteen experiments were performed, at pressures included between 27.208 and 12.583 inches of mercury, and the results were found in very close accordance with his formula; but less than the numbers deducible from the theoretic expression

$\gamma^* = c \left(\frac{P}{p} \right)^{-\frac{1}{k}}$, given by Poisson in his *Traité de Mécanique*, tom. ii. p. 649.

XLVII. *Chemical Analysis of Meteoric Iron, from Claiborne, Clarke County, Alabama.* By CHARLES T. JACKSON.†

Aug. 5, — MR. F. ALGER handed me this remarkable 1834. — mineral, which he had received from Mr. Hubbard, who had obtained the specimen during his travels in Alabama, and thought, from the bright streaks in it, that it might be an ore of silver.

On examining this substance, it soon appeared that it was different from any metallic ore of terrestrial origin, and that it is a very peculiar and remarkable meteorite.

Having surmised its probable origin, I was desirous of seeing the gentleman who brought it from Alabama, and at the request of Mr. Alger, Mr. Hubbard called upon me and gave me the following particulars as to its locality.

He found the specimen on the surface of the earth, near Lime Creek, in Claiborne, Alabama. The soil at that place is composed of red marl, or clay, and the rocks in place are sandstones, mostly of a gray colour. The mass from which my specimen was broken, was of an irregular triangular shape, rounded at the corners, and was 10 inches long by 5 or 6 inches in thickness. It was extremely heavy, insomuch that he could not conveniently carry with him the whole mass, and therefore employed a negro to break it with a sledge-hammer; which operation proving too difficult for him, Mr. Hubbard took the sledge himself, and with the cutting edge, by many hard blows, he ultimately succeeded in detaching the portion in my possession. It is much to be regretted that he did not bring with him the whole mass, and I desired him to send for the remainder, but have not yet heard from him. He is of opinion, that there are many other similar masses

* In this expression γ is the specific heat of air under a constant pressure when the height of the barometer is p , c the same when the height is P , and K is the ratio between the specific heat of air under a constant volume and a constant pressure.

† From Silliman's *American Journal of Science and Arts*, vol. xxxiv. p. 332.

near the spot where this was found; but it is not probable that they abound to the extent imagined. I beg leave, however, to call the attention of travellers to the locality mentioned, where the remainder of the specimen still exists neglected.

Description of the Specimen.—It is of an irregular form, rounded upon all the sides excepting on that where it was fractured, which presents a rough hackly surface, with projecting, bright, silvery streaks, and deep greenish and brown eroded surfaces, from which an exudation of green liquid takes place, on exposing the specimen to moist air.

The rounded surface is coated with a thin layer of the *subchloride of iron*, which being removed, the mass is found to consist of *metallic matter*, resembling wrought iron, when the specimen is filed bright. On attempting to break off a fragment, the mass was found to be extremely tough and malleable, so as to require the aid of a file and cutting-chisel.

Sp. gr. on three separate fragments from different parts of the mass, 5.750, 6.400 and 6.500. The whole mass weighs 28 ounces avoirdupois.

Having washed the specimen in distilled water several times, I filed one side of it bright, and left it exposed to the air in my cabinet. In a few days, numerous grass-green drops of liquid began to collect on its surface, and became externally coated with a thin brown film. This liquid had a slight alkaline astringent taste, but gave no alkaline reaction with turmeric paper or Brazil wood solution. A few drops collected in a test tube and diluted with water, gave an *abundant thick curdy white precipitate*, with a solution of *nitrate of silver*, showing the presence of chlorine in combination. *Ferro-cyanate of potash* gave a *blue precipitate*, indicative of iron, and *ammonia* gave a precipitate of the *hydrated peroxide of iron*. Muriate of ammonia having been added to a little more of the exudation, the peroxide of iron was precipitated by ammonia, and the remaining liquid was of a pale blue colour, indicative of nickel, and on addition of pure potash, hydrate of nickel formed in a bulky green precipitate.

Thus the green drops in question were proved to be composed of the hydro-chlorates of nickel and iron, and they doubtless form from the action of the moisture of the atmosphere upon the metallic chlorides contained in the meteorite.

Analysis of the mass.—Several fragments of the specimen having been cut off by means of a steel chisel and hammer, their specific gravities were ascertained, and they were then subjected to analysis.

Specimen 1. A fragment weighing 25 grains, sp. gr.

= 5.750, being placed in a green glass flask, and pure nitric acid poured upon it, no action took place until heat was applied, when a violent effervescence, with extrication of nitrous acid fumes, began, and the solution was rapidly and entirely effected. The solution was then treated with a sufficient quantity of the solution of muriate of ammonia, to prevent the precipitation of the nickel, and then the peroxide of iron was thrown down by means of liquid ammonia. When the precipitate had subsided, the whole was thrown on a filter, and the peroxide of iron was thoroughly washed, dried, ignited in platina capsule, and weighed = 23.5 grs. peroxide of iron = 16.296 grs. metallic iron.

The solution, which had passed the filter, was of a clear blue colour, with a slight amethystine tint, indicative of nickel. This solution and the mingled washings were evaporated in a glass vessel to a small bulk, and then treated, while warm, with a hot solution of pure potash, when a dense bulky green precipitate of the hydrate of nickel was thrown down, which being collected on a filter, washed, thoroughly dried and ignited in a platina crucible, weighed 8.8 grains = oxide of nickel = 6.927 grains metallic nickel.

Analysis—2d specimen. A fragment of the meteorite, weighing 50 grains, was found to have a sp. gr. = 6.500.

It was placed in a green glass flask, perfectly pure nitric acid was poured upon it, and heat was gradually applied until the solution was completed. It was then diluted with pure distilled water, and a solution of nitrate of silver was added, when an abundant curdy white precipitate of chloride of silver took place. When the operation was complete, I filtered the solution, collected the washed chloride of silver, and dried and fused it in a small porcelain capsule. It weighed = 3 grains = chloride of silver = 0.74 gr. chloride, or 0.76 hydrochloric acid.

The solution was then cleared of nitrate of silver, by means of hydro-chloric acid, and filtered. Then muriate of ammonia being added, the peroxide of iron was precipitated by pure ammonia, and after washing, drying, and ignition, weighed = 48 grains = 33.28 grs. metallic iron.

The oxide of nickel was precipitated by means of a solution of pure potash, and when collected, washed, dried, and ignited, weighed 15.8 grains oxide of nickel = 31.6 per cent. = 24.708 per cent. metallic nickel. After the separation of the metallic oxides, the solution was treated by means of a solution of acetate of barytes, and a white precipitate of sulphate of barytes was formed, which weighed, after washing and drying, = 27 grains = 2 grs. sulphur.

The presence of chrome and of manganese having been indicated, I took a separate portion of the meteorite, weighing 10 grains, dissolved it in hydro-chloric acid, adding sufficient tartaric acid to retain the oxides in solution, neutralized the acid by ammonia, and precipitated the iron and nickel, by means of a current of hydro-sulphuric acid gas; after filtration, I evaporated the solution to dryness and burned off the tartaric acid in a small platina capsule under the muffle, when a small quantity of chromic acid was obtained, which was recognised by its characters before the blowpipe; its amount was estimated at 3 per cent. The manganese is also estimated.

From the above analyses, it will appear that specimen 1st of the meteoric iron, having a sp. gr. of 5.750, contains in 25 grains,

Metallic iron 16.296 = 65.184 per cent.

„ nickel..... 6.927 = 27.708 „

And in specimen 2nd, having a sp. gr. of 6.500 in 50 grains we have or in 100 grains.

Metallic iron 33.280 66.560

„ nickel 12.354 24.708

„ chrome and manganese 1.625 3.240

„ sulphur 2.000 4.000

„ chlorine740 1.480

49.999 99.988

It will be remarked, that this meteorite contains an unusual proportion of nickel, and that the occurrence of chlorine, in matter of celestial origin, is here noticed for the first time.

I beg leave therefore to invite chemists to a careful review of meteorites, since the occurrence of chlorine may have been overlooked in former analyses.

Its occurrence in meteoric matters is a fact of great importance, in accounting for their chemical phænomena, while passing through our atmosphere.

It must also be remembered, that chloride of iron is readily volatilized at a high temperature, and that it is abundantly exhaled from the craters of volcanos, in various parts of our planet.

Nickel, however, has not to my knowledge been discovered amid volcanic sublimations, but it may be worth while to call the attention of chemists to the subject, that it may be sought for in volcanic craters.

I am however far from believing that we shall be able to prove that all meteorites originate from volcanic sublimations,

for there are very evident reasons for believing that our planet, stately in its course, passes amid numerous detached masses of matter or asteroids, which regularly meet the earth in its orbit on the 13th of November; at least such are the views of Prof. Olmsted, of Arago and Gay-Lussac, whose opinions appear to be supported by the facts which they have collected.

Allowing that meteoric matters are projected from cometary masses, which stately cross the earth's orbit, coming within the limits of its attraction, and are subjected to the oxidizing influence of the atmosphere, so as to take fire and fall in burning masses upon the surface of the earth, we can more readily account for the phenomena exhibited in their splendid coruscations, when we know that the meteors contain ingredients possessing remarkable decomposing powers, if brought into contact with water or aqueous vapour, and such are the effects of the chlorides of iron and nickel.

In several instances on record, we find the meteor first discovering itself, bursting into fire, from the midst of a dark cloud, and throwing off brilliant coruscations of light, and ejecting ignited masses which fall to the earth; while the globe of fire, from which they were thrown off, traverses the heavens, and gradually becomes extinct. May not therefore the moisture of the atmosphere have first kindled the meteor in its passage through the humid clouds? I do not know whether they are generally too distant from the earth to come in contact with clouds, but from the rapidity of these apparent meteors they cannot be very distant, at the moment of their conflagration. Should chlorine prove to be a common or constant ingredient, I suppose, that we should have a ready solution of the phenomena involved in the problem.

With respect to the specimen, which forms the subject of the present communication, if we consider its chemical composition, we are forced to regard it of celestial origin; for we have no similar natural alloy in this world, and it contains elements, which are generally found in meteoric matters, besides the new ingredient which I have discovered as one of its components. It is clearly impossible that this mass should have been factitious; for in all manufactured iron, we can readily detect carbon, which does not exist in our specimen, and the situation in which it was found is presumptive evidence that it was not manufactured, and the rocks around, not belonging to the class bearing metallic ores, it is impossible for it to have been derived from them, and it could not have been derived from the distant rocks by diluvial transportation, for no such ores exist in any of our mines.

Had it been an ore of iron, reduced by a blast of lightning, we should not have found it alloyed with nickel.

We are therefore led to conclude, that our specimen is of celestial origin, and that it is a fragment of one of those asteroids of cometary matter, which wandering in space, occasionally cross our orbit, and being attracted by the earth, so that they rush through our atmosphere, bursting into fire and descending, take up their abode on this sublunary sphere.

Boston, May 29, 1838.

XLVIII. *Experimental Researches in Electricity*.—*Eleventh Series*. By MICHAEL FARADAY, Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

[Continued from p. 299.]

¶ iv. *Induction in curved Lines*.

1215. **A**MONGST those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge, (though they have long been to me indications of prior molecular action (1230.)) I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

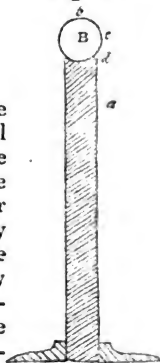
1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines.

It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0.9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and Coulomb's electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360° , and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated*, separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (fig. 3. and 4.) observed in succession, was as follows:

| | |
|---------------------------|--------------------|
| at <i>a</i> | above 1000° |
| <i>b</i> it was | 149 |
| <i>c</i> | 270 |
| <i>d</i> | 512 |
| <i>b</i> | 130 |

Fig. 3.



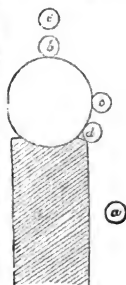
1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell-lac cylinder; for whatever electricity the ball B received by *communication* from the shell-lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as

* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places, that would have given to it, if uninsulated, a different condition.

might be expected from an inductive action in straight lines, but that obtained at *b* is *not so*: it is clearly a charge by induction, but *induction in a curved line*; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from *B*, could not, in consequence of the size of *B*, be connected by a straight line with any part of the excited and inducing shell-lac.

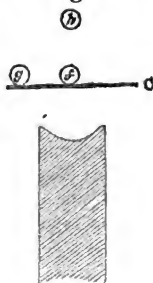
1220. To suppose that the upper part of the *uninsulated* ball *B*, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.); and though some persons may not be prepared as yet to admit this with respect to insulated conductors, all will as regards uninsulated conductors like the ball *B*; and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with *B*, un-insulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state: it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of *B*.

Fig. 4.



1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball *B* a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface; but

Fig. 5.



if the plate be enlarged to about $1\frac{1}{4}$ or 2 inches in diameter, *C* (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even above the middle at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold-leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball *B* (fig. 6.) so as to keep it

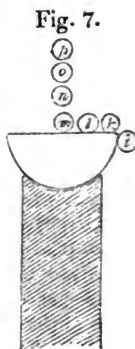
in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phenomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.



1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was 1.36

of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was 112° , at *k* 108° , at *l* 65° , at *m* 35° ; the inductive force gradually diminishing, as might have been expected, to this point. But on raising the carrier to the position *n* the charge increased to 87° ; and on raising it still higher to *o*, the charge still further increased to 105° : at a higher point still, *p*, the charge taken was smaller in amount, being 98° , and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilinear form by the shape, position, and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another: all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.



1225. As another proof that the whole of these actions were inductive, I may state a result which was exactly what might be expected, namely, that if uninsulated conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and

could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phenomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined towards them that the lateral tension or mutual repulsion of the lines of force before spoken of, (1224.) by which their inflexion is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions *k, l, m, n, o, p*, (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substitution of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac, (1272.) and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (1224.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0.6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at *n, o, p*, and *q*. Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received. This was done by breathing and wiping (1203.),

and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same: and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negatived.

1229. In these experiments with solid dielectrics, the degree of charge, assumed by the carrier ball at the situations *n, o, p* (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B,

Fig. 9.

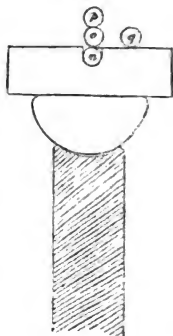


Fig. 8.



or even a plate, provided the edges be not too thin, be held before it, a small electrometer at *c* or at *d*, uninsulated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by Nicholson*, who, however, reasons erroneously upon it. As to its

* *Encyclopædia Britannica*, vol. vi. p. 504.

introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its statical state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductric and the inductive surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the

charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

1234. One of the inductive apparatus already described (1187, &c.) had a hemispherical cup of shell-lac introduced, which being in the interval between the inner ball and the lower hemisphere, nearly occupied the space there; consequently when the apparatus was charged, the lac was the dielectric or insulating medium through which the induction took place in that part. When this apparatus was first charged with electricity (1198.) up to a certain intensity, as 400° , measured by the Coulomb's electrometer (1180.), it sank much faster from that degree than if it had been previously charged to a higher point, and had gradually fallen to 400° ; or than it would do if the charge were, by a second application, raised up again to 400° ; all other things remaining the same. Again, if after having been charged for some time, as fifteen or twenty minutes, it was suddenly and perfectly discharged, even the stem having all electricity removed from it (1203.), then the apparatus being left to itself, would gradually recover a charge, which in nine or ten minutes would rise up to 50° or 60° , and in one instance to 80° .

1235. The electricity which in these cases returned from an apparently latent to a sensible state, was always of the same kind as that which had been given by the charge. The return took place at both the inducing surfaces; for if after the perfect discharge of the apparatus the whole was insulated, as the inner ball resumed a positive state the outer sphere acquired a negative condition.

1236. This effect was at once distinguished from that produced by the excited stem acting in curved lines of induction (1203. 1232.), by the circumstance that all the returned electricity could be perfectly and instantly discharged. It appeared to depend upon the shell-lac within, and to be, in some way, due to electricity evolved from it in consequence of a previous condition into which it had been brought by the charge of the metallic coatings or balls.

1237. To examine this state more accurately, the apparatus, with the hemispherical cup of shell-lac in it, was charged for about forty-five minutes to above 600° with positive elec-

tricity at the balls *h* and *B* (fig. 1.) above and within. It was then discharged, opened, the shell-lac taken out, and its state examined; this was done by bringing the carrier ball near the shell-lac, uninsulating it, insulating it, and then observing what charge it had acquired. As it would be a charge by induction, the state of the ball would indicate the opposite state of electricity in that surface of the shell-lac which had produced it. At first the lac appeared quite free from any charge; but gradually its two surfaces assumed opposite states of electricity, the concave surface, which had been next the inner and positive ball, assuming a positive state, and the convex surface, which been in contact with the negative coating, acquiring a negative state; these states gradually increasing in intensity for some time.

1238. As the return action was evidently greatest instantly after the discharge, I again put the apparatus together, and charged it for fifteen minutes as before, the inner ball positively. I then discharged it, instantly removing the upper hemisphere with the interior ball, and, leaving the shell-lac cup in the lower uninsulated hemisphere, examined its inner surface by the carrier ball as before (1237.). In this way I found the surface of the shell-lac actually *negative*, or in the reverse state to the ball which had been in it; this state quickly disappeared, and was succeeded by a positive condition, gradually increasing in intensity for some time, in the same manner as before. This first negative condition of the surface opposite the positive charging ball is a natural consequence of the state of things, the charging ball being in contact with the shell-lac only in a few points. It does not interfere with the general result and peculiar state now under consideration, except that it assists in illustrating in a very marked manner the ultimate assumption by the surfaces of the shell-lac of an electrified condition, similar to that of the metallic surfaces opposed to or against them.

1239. *Glass* was then examined with respect to its power of assuming this peculiar state. I had a thick flint glass hemispherical cup formed, which would fit easily into the space *o* of the lower hemisphere (1188. 1189.); it had been heated and varnished with a solution of shell-lac in alcohol, for the purpose of destroying the conducting power of the vitreous surface. Being then well warmed and experimented with, I found it could also assume the *same state*, but not apparently to the same degree, the return action amounting in different cases to quantities from 6° to 18° .

1240. *Spermaceti* experimented with in the same manner gave striking results. When the original charge had been

sustained for fifteen or twenty minutes at about 500° , the return charge was equal to 95° or 100° , and was about fourteen minutes arriving at the maximum effect. A charge continued for not more than two or three seconds was here succeeded by a return charge of 50° or 60° . The observations formerly made (1234.) held good with this substance. Spermaceti, though it will insulate a low charge for some time, is a better conductor than shell-lac, glass, and sulphur; and this conducting power is connected with its readiness in exhibiting the particular effect under consideration.

1241. *Sulphur*.—I was anxious to obtain the amount of effect with this substance, first, because it is an excellent insulator, and in that respect would illustrate the relation of the effect to the degree of conducting power possessed by the dielectric (1247.); and in the next place, that I might obtain that body giving the smallest degree of the effect now under consideration, for the investigation of the question of specific inductive capacity (1277.).

1242. With a good hemispherical cup of sulphur cast solid and sound, I obtained the return charge, but only to an amount of 17° or 18° . Thus glass and sulphur, which are bodily very bad conductors of electricity, and indeed almost perfect insulators, gave very little of this return charge.

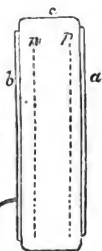
1243. I tried the same experiment having *air* only in the inductive apparatus. After a continued high charge for some time I could obtain a little effect of return action, but it was ultimately traced to the shell-lac of the stem.

1244. I sought to produce something like this state with one electric power and without induction; for upon the theory of an electric fluid or fluids, that did not seem impossible, and then I should have obtained an absolute charge (1169. 1177.), or something equivalent to it. In this I could not succeed. I excited the outside of a cylinder of shell-lac very highly for some time, and then quickly discharging it (1203.), waited and watched whether any return charge would appear, but such was not the case. This is another fact in favour of the inseparability of the two electric forces, and another argument for the view that induction and its concomitant phenomena depend upon a polarity of the particles of matter.

1245. Although inclined at first to refer these effects to a peculiar masked condition of a certain portion of the forces, I think I have since correctly traced them to known principles of electrical action. The effects appear to be due to an actual penetration of the charge to some distance within the electric, at each of its two surfaces, by what we call conduction; so that, to use the ordinary phrase, the electric

forces sustaining the induction are not upon the metallic surfaces only, but upon and within the dielectric also, extending to a greater or smaller depth from the metal linings. Let c (fig. 10.) be the section of a plate of any dielectric, a and b being the metallic coatings; let b be uninsulated, and a be charged positively; after ten or fifteen minutes, if a and b be discharged, insulated, and immediately examined, no electricity will appear in them; but in a short time, upon a second examination, they will appear charged in the same way, though not to the same degree, as they were at first. Now suppose that a portion of the positive force has, under the coercing influence of all the forces concerned, penetrated the dielectric and taken up its place at the line p , a corresponding portion of the negative force having also assumed its position at the line n ; that in fact the electric at these two parts has become charged positive and negative; then it is clear that the induction of these two forces will be much greater one towards the other, and less in an external direction, now that they are at the small distance np from each other, than when they were at the larger interval ab . Then let a and b be discharged; the discharge destroys or neutralizes all external induction, and the coatings are therefore found by the carrier ball unelectrified; but it also removes almost the whole of the forces by which the electric charge was driven into the dielectric, and though probably a part goes forward in its passage and terminates in what we call discharge, the greater portion returns on its course to the surfaces of c , and consequently to the conductors a and b , and constitutes the re-charge observed.

Fig. 10.



1246. The following is the experiment on which I rest for the truth of this view. Two plates of spermaceti, d and f (fig. 11.), were put together to form the dielectric, a and b being the metallic coatings of this compound plate, as before. The system was charged, then discharged, insulated, examined, and found to give no indications of electricity to the carrier ball. The plates d and f were then separated from each other, and instantly a with d was found in a positive state, and b with f in a negative state, nearly all the electricity being in the linings a and b . Hence it is clear that, of the forces sought for, the positive was in one half of the compound plate and the negative in the other half; for when removed bodily with the plates from each other's inductive in-

fluence, they appeared in separate places, and resumed of necessity their power of acting by induction on the electricity of surrounding bodies. Had the effect depended upon a peculiar relation of the contiguous particles of matter only, then each half plate, *d* and *f*, should have shown positive force on one surface and negative on the other.

1247. Thus it would appear that the best solid insulators, such as shell-lac, glass, and sulphur, have conductive properties to such an extent, that electricity can penetrate them bodily, though always subject to the overruling condition of induction (1178.). As to the depth to which the forces penetrate in this form of charge of the particles, theoretically, it should be throughout the mass, for what the charge of the metal does for the portion of dielectric next to it, should be done by the charged dielectric for the portion next beyond it again; but probably in the best insulators the sensible charge is to a very small depth only in the dielectric, for otherwise more would disappear in the first instance whilst the original charge is sustained, less time would be required for the assumption of the particular state, and more electricity would re-appear as return charge.

1248. The condition of *time* required for this penetration of the charge is important, both as respects the general relation of the cases to conduction, and also the removal of an objection that might otherwise properly be raised to certain results respecting specific inductive capacities, hereafter to be given (1269. 1277.).

1249. It is the assumption for a time of this charged state of the glass between the coatings in the Leyden jar, which gives origin to a well-known phenomenon, usually referred to the diffusion of electricity over the uncoated portion of the glass, namely, the *residual charge*. The extent of charge which can spontaneously be recovered by a large battery, after perfect uninsulation of both surfaces, is very considerable, and by far the largest portion of this is due to the return of electricity in the manner described. A plate of shell-lac six inches square, and half an inch thick, or a similar plate of spermaceti an inch thick, being coated on the sides with tin-foil as a Leyden arrangement, will show this effect exceedingly well.

1250. The peculiar condition of dielectrics which has now been described, is evidently capable of producing an effect interfering with the results and conclusions drawn from the use of the two inductive apparatus, when shell-lac, glass, &c. are used in one or both of them (1192. 1207.); for upon dividing the charge in such cases according to the method described

(1198. 1207.), it is evident that the one just receiving its half charge must fall faster in its tension than the other. For suppose app. i. first charged, and app. ii. used to divide with it; though both may actually lose alike, yet app. i., which has been diminished one half, will be sustained by a certain degree of return action or charge (1234.), whilst app. ii. will sink the more rapidly from the coming on of the particular state. I have endeavoured to avoid this interference by performing the whole process of comparison as quickly as possible, and taking the force of app. ii. immediately after the division, before any sensible diminution of the tension arising from the assumption of the peculiar state could be produced; and I have assumed that as about three minutes pass between the first charge of app. i. and the division, and three minutes between the division and discharge, when the force of the non-transferable electricity is measured, the contrary tendencies for those periods would keep that apparatus in a moderately steady and uniform condition for the latter portion of time.

1251. The particular action described occurs in the shell-lac of the stems, as well as in the *dielectric* used within the apparatus. It therefore constitutes a cause by which the outside of the stems may in some operations become charged with electricity, independent of the action of dust or carrying particles (1203.).

[To be continued.]

XLIX. *Justification of the Contact Theory of Galvanism.*

By G. TH. FECHNER.

[Continued from p. 217.]

II. *Facts which relate to the closed circuit.*

1. **I**T appears according to the theory of contact as if iron, which when brought into contact with copper in water or diluted acids exhibits positive electricity, must on the contrary be positive in all other fluids; it acts however, nevertheless, negatively in a solution of sulphuret of potassium (*Schwefelleber*). Other examples may be placed by the side of this, in which also the electromotive condition of the metals changes with the consistence of the intermediate fluid, for instance, that of tin and copper is in solution of ammonia the reverse of what it is in pure water, and that of copper and lead, in concentrated nitric acid, is the opposite of that in diluted nitric acid, which immediately obviates the objection,

that the different electromotive action of the various fluids upon the metals should call forth of itself an apparent reversion of the polarity, since in the latter experiment the fluid, disregarding the degree of concentration, remains the same. According to the chemical view all these cases are easily explained by the general rule, that that metal which is most strongly acted upon by the fluid, is always the positive one (De la Rive in his *Recherches*, p. 7.).

To this objection I believe I have paid sufficient attention in a detailed memoir respecting the reversions of polarity of the circuit*, and with which undoubtedly De la Rive cannot be acquainted. I have shown that when copper in a solution of sulphuret of potassium is positive with respect to iron, this is caused by a change (perceptible even to the eye) of its surface, which takes place *immediately* on immersion in a concentrated, *gradually* on immersion in a diluted solution of sulphuret of potassium, whence therefore in a diluted solution the normal divergence also first takes place, and then gradually passes over into the opposed. Besides, the copper, changed in the solution of sulphuret of potassium, is even positive towards iron in other fluids. I have further proved that even in other concentrated fluids and with other metals, in which an electromotive state opposed to the general one may be observed, in general there exists a certain degree of dilution of the fluid, below which the metals indicate at the beginning the normal state, which then gradually passes over into the opposite (if the solution be not too dilute), which also argues a change gradually produced in the surfaces of the metals, although it cannot in every case be demonstrated in a direct manner. I have also shown by other different experiments that in these cases the change of the fluid has no influence on the effects. If, moreover, the positive metal happens to be the one which is also always acted upon most strongly, then this coincidence of the two circumstances would in no degree determine which of the two was to be considered as the cause of the other; as, however, the cases given in my memoir did not include those specifically related by De la Rive of the reverse action of tin and copper in ammonia and of lead and copper in concentrated nitric acid, I have lately convinced myself that in fact, if we unite tin and copper in officinal *liquor ammoniac*, lead and copper in concentrated nitric acid, even when these liquids are not diluted, the divergence in the beginning indicates the normal positive condition respectively of the tin and the lead, which however after a short time (accordingly as the change

* Schweigger's *Journal*, vol. liii. p. 61—129, or Biot's *Lehrbuch* (Precis) vol. iii. p. 93.

takes place in the metals) passes over into the negative. The taking place of the change of the metals in these fluids may also be proved by various other experiments, which however I here omit as they may be brought forward in connexion much better in another place.

The changes which fluids produce in metals have certainly not been sufficiently attended to and studied, and I shall often have occasion in this memoir to return to this point. All the experiments which Pfaff, Karsten, Becquerel, and others have made on the production of electricity between fluid and solid bodies are at least complicated by these changes and demand in this respect a revision. It is not at all impossible that the result of these experiments depends entirely on such changes. Further experiments will, I hope, soon solve this part of the question.

2. Schœnbein has published in these *Annalen* (vol. xxxix. p. 351*) an experiment which he considers to be especially decisive in favour of the chemical theory, and which chiefly amounts to this, that an iron wire rendered passive by previous immersion in nitric acid, then connected with the circuit in a solution of sulphate of copper by means of a platinum wire, precipitates no copper and produces no current, but that a current immediately takes place if by any cause the passive condition of the wire is destroyed, *i. e.* if its chemical state is again restored.

With respect to this experiment we may state the following:

By previous experiments performed by Wetzlar, and lately by myself, it has been sufficiently shown that the change (as little explained in the sense of the chemical theory as in that of the contact theory) which iron undergoes in nitric acid and in a solution of concentrated nitrate of silver, renders it considerably more negative than previously. That a highly negative metal neither precipitates copper, nor gives with platinum any perceptible current, (especially if the metals are employed in the form of wire, and a multiplier is made use of, of little sensibility,) certainly cannot be regarded as incompatible with the theory of contact, and as little, that a current immediately re-appears with the precipitation of the copper, if by any cause that change of surface has been destroyed. Where is the proof that the precipitation of the copper is not rather the consequence than the cause of the restored electrical action? Besides, it is possible that the peculiar change which iron undergoes in nitric acid may also increase its

[* See also L. and E. Phil. Mag., vol. x. p. 275; and also p. 167 of the present volume.]

power of opposing the transfer of electricity. (Compare with No. 3.) I cannot find the least proof in this experiment. From the same point of view we must also consider the experiment published by De la Rive in these *Annalen*,[†] vol. xl. p. 368. If I say that iron also undergoes a change of the same kind in caustic potash, that it becomes considerably more negative by its action than before, all that is there stated appears to me explained, although the change of the iron itself may still need an explanation not less when regarded according to the chemical theory than to the theory of contact. Among other circumstances, we find a proof of this change, that iron connected with copper in caustic potash to form a circuit, at first gives the normal divergence of the multiplier, which divergence makes the iron appear positive, but shortly after it changes. That, however, iron gives no current with platinum in caustic potash I did not find to be the case. On applying a very delicate multiplier the current was strong enough to place the double needle almost perpendicular to the coils.

3. The following experiments belong to those which at first sight appear most strikingly to favour the chemical theory, and to which its supporters attach especial importance. Zinc with copper gives in distilled water, as also in concentrated sulphuric acid, closed, only weak electric currents; in diluted sulphuric acid, a mixture of both fluids, very powerful ones. Since it is not probable that the proper conducting power of the fluids is changed by their mixture, there remains nothing more than to attribute the increased current to the increased chemical action which takes place on diluting the sulphuric acid, which produces the increased development of electricity. Similar to this is the following experiment: platinum with gold gives in pure nitric acid, as also in pure muriatic acid, a weak and transient, according to De la Rive's statement, an imperceptible current, but in aqua regia an apparently strong one. Many similar examples are mentioned which all belong to the same class. However, supposing that the mixture of the fluids does not change their conducting power*, it is still questionable whether the increased effect in all these cases produced by increased chemical action does not depend merely on this, that the opposition to the transfer decreases with the increase of chemical action. In fact I have shown in my galvanic measurements that this opposition is smaller in power-

[* It appears to us, on the contrary, that the chemical action which ensues on the mixture of the nitric and muriatic acids, by which they are both, in part, decomposed, must effect a corresponding change in their conducting power.—EDIT.]

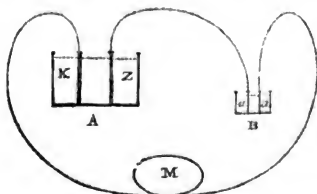
fully acting fluids. The following experiments will probably be more decisive with respect to the subject now before us.

If I bring a fluid between homogeneous plates, a current ought not to originate according to any theory, on connecting the plates with the multiplier, from the independent action of this system; or if I *interpose* a fluid between homogeneous plates in an active circuit, this interposition would, according to each theory, influence the force of the circuit in no other manner than by its opposing power as long as the plates are unchanged. It then appears:—

a. That concentrated sulphuric acid inserted in an active circuit between homogeneous zinc plates or muriatic acid between homogeneous platinum plates, considerably weakens the force of this circuit; a proof that a strong opposition to the conduction has arisen in the circuit.

b. That in like manner distilled water inserted in the circuit between homogeneous zinc plates or nitric acid between homogeneous platinum plates considerably diminishes its power.

c. That, however, diluted sulphuric acid inserted in the circuit between homogeneous zinc plates, or a mixture of muriatic and nitric acids between homogeneous platinum plates, weaken the power but little in proportion; a proof that from the mixture (and that undoubtedly from the excited chemical reaction) of the fluids the opposition to the conduction has really diminished. The details of the experiments are as follows.



In the apparatus A a zinc and copper circuit K Z (of each plate 11·2 Parisian square inches immersed at 4 lines distance) was connected in highly acidulated water with the multiplier M (consisting of thick copper wire but not of many coils), and the interposed intermediate apparatus which contained the zinc plates *aa*, and either water or fuming sulphuric acid, or diluted sulphuric acid, as the exciting fluid. The distance of the plates *aa* was 14 lines, the immersed surface 1·1 square inch. The measure of the power of the current was always taken by the first oscillations of the needle, which was placed

perpendicular to the coils, immediately after the closing, by means of the method already sufficiently described elsewhere; and the circuit, immediately re-opened in order to avoid as much as possible the decrease of action. Moreover, before each new measurement the active condition of the zinc and copper circuit was not only restored by being kept long out, (vide on this subject my measurements of the galvanic circuit,) but even the zinc plates *aa*, were cleaned by filing. If the power of the current without the interposed apparatus was represented by 1000, it became on the insertion of the intermediate apparatus :

| | |
|---|----------|
| When the interposed apparatus was filled with | |
| distilled water | } 0.8275 |
| _____ with fuming sulphuric acid | 64.685 |
| _____ with diluted sulphuric acid | 851.04 |

The last power could not be accurately measured on account of the extreme velocity of the first oscillations, and probably approaches much closer to the power without the interposed apparatus than as stated.

For the preparation of the dilute sulphuric acid I took, by estimation, 1 volume of fuming sulphuric acid and 3 volumes of distilled water.

On employing platinum instead of zinc plates in the interposed apparatus, I obtained the following relative current forces according to the fluids it was charged with, by which, the value of the current without the interposed apparatus being again fixed at = 1000,

| | |
|--|----------|
| When the apparatus was filled with concen- | |
| trated nitric acid | } 17.703 |
| _____ with muriatic acid | 2.5822 |
| _____ with nitro-muriatic acid | 338.34 |

The nitro-muriatic acid was composed of about equal volumes of each acid (I did not however take the sp. gr.). It is remarkable that the action in the nitro-muriatic acid did not immediately after closing the circuit attain the assigned magnitude, but was considerably weaker, but very quickly rose to that magnitude, which circumstance I frequently observed; while, in the experiment with zinc, sulphuric acid and water, the power here stated was attained immediately. The change, on which this increase depended, was an action of the closure itself, since it was even indicated when the platinum plate had already stood for some time in nitric acid previously to the closing, and was repeated at new closings after intervening breaks. In the mean time it cannot be ascribed to a gradual charging of the plate so far as by that is understood a change of the electro-motive power, since by

this the force of the circuit would only have decreased; for the platinum plate communicating metallically with the copper, indicated itself, not only by a development of gas bubbles in the circuit, but even by a distinct experiment, the circuit being made with it after some lapse of time between the closings, as negative with relation to the copper. The cause then of the gradually increasing action could only be sought for in the decrease of opposition gradually taking place.

The experiment with the zinc plates was performed twice, that with the platinum plates three times, on different days, with results exactly corresponding. Moreover De la Rive affirms that platinum connected in pure nitric acid with pure gold produces no current, or rather one which quickly disappears, which he ascribes to accidental impurities. I have not been able any more than Marianini to find this circumstance confirmed; although I employed gold of 24 carats, and a nitric acid which when properly diluted was not rendered turbid by nitrate of silver. Fresh surfaces were given to the metals by rubbing them with dry sand-paper (I expressly avoided treating them with acids to clean them, because in this case peculiar variations of surfaces are to be feared). The divergence which at the beginning was very lively, decreased, it is true, considerably, very soon, but remained still constantly in favour of the positive relation of the gold. A very sensitive multiplier of several thousand coils, however, was employed. With the same instrument I have observed an action not inconsiderable between gold and platina in rectified muriatic acid, which however with a multiplier of a medium number of coils but of thicker wire, was scarcely perceptible. The reasons from Ohm's theory may be sufficiently well known with us to lead us to expect, that, in fact, in this case, where the circuit includes in itself a considerable opposition, we can expect action only from a multiplier with *numerous* coils, even when of thin wire.

Notwithstanding, however, that De la Rive, under the circumstances of his experiment, could observe no action of a gold and platinum circuit, he would undoubtedly have observed it, if he had saturated this acid with nitrate of silver in excess, in which case, in fact, in all appearance opposed to the chemical theory, a not less considerable divergence is obtained than if gold and platinum are connected in a solution of nitrate of silver only. I am, however, far from giving these circumstances an importance against the chemical theory, equivalent to that which experiments of apparently an opposed kind are made to have in favour of it, for it is certain that a solution of nitrate of silver exerts a modified influence on the surfaces of gold and platinum, although this is op-

posed to the common notions of chemistry. It now remains only to determine whether this changing action of the fluid on the metal is also the cause of the current. That a solution of nitrate of silver really effects a change of electrical state in gold and platinum, will be seen from the following circumstances:

Gold is negative to platinum in a concentrated solution of nitrate of silver (1 part salt to 8 water) as also in a solution in some degree diluted, and finally also in nitric acid saturated to excess with a solution of nitrate of silver; on employing a solution of nitrate of silver greatly diluted a reversion occurs

from platinum $\overset{+}{\text{gold}}$ into platinum $\overset{-}{\text{gold}}$. There are many other proofs of the resulting change of the platinum to positive, which I shall here however neglect. Moreover, my experiments have taught me that even the noble metals are capable of undergoing the most remarkable electro-chemical changes in fluids which have generally been considered as devoid of action upon them. It would however lead us too far were we to treat on this subject at present.

It is evident from all of the above, that with the present state of science experiments on the weaker or smaller galvanic activity of circuits in fluids of apparently greater or smaller chemical activity cannot give certain results either favourable or unfavourable to the chemical theory. The principal object is to discover the cause of the peculiar changes which in many cases the general condition of the metals would not lead us to expect, that metals undergo in fluids, before we can make use of the condition so acquired for the explanation of other circumstances.

4. The following experiment against the chemical theory I made known a long time ago in Schweigger's *Journal*, vol. lvii. p. 9, and I should not publish it anew if it had been noticed by any one of the supporters of that theory, which makes me suppose that it is but little known. This experiment, so easily repeated, and which I annually perform as a class experiment, appears to me to be exactly an *experimentum crucis* against the chemical theory.*

Let an equal number of pairs of zinc and copper plates (I generally employ ten) be arranged in a *couronne des tasses* so as to form a compound circuit arranged as a pile; so, however, that the one half of the elements tends to produce a current opposed to that of the other. Let the conducting fluid be water. If everything is the same in all the cells, the two opposite currents will compensate each other in action at the connecting multiplier and will produce no divergence. It

* See Prof. Schœnbein's remarks on this experiment, in p. 162.

happens at times that current equilibrium is obtained very exactly, and then this equilibrium continues even when any quantity of muriatic acid is added to the fluid contained in one of the systems; the one system may even be filled much higher with dilute acid than that producing the opposite current is with water. It is true that an ascendancy of the one current is gradually developed, undoubtedly in consequence of the changing action of the muriatic acid upon the plates, *but it is not the cells with acid, in which a tumultuous development of gas takes place, but the water-cells which obtain this ascendancy.* If on the other hand each half of the cells is connected by itself by the multiplier, that instrument experiences from the acid cells a very stormy action, from the water-cells a weak action merely. How then the result of this experiment is to be explained according to the chemical theory, I cannot conceive. According to the theory of contact the explanation is easy. For according to this the addition of muriatic acid increases the action only by diminishing the opposition to the conduction present in the circuit, and this diminution is of as great advantage to the electricity (which is developed by contact in the cells without acid,) in its entire circulation through the circuit, as the electricity of the pairs of plates which are in the very acid fluid. We do not always succeed in meeting with such homogeneous plates, that when in the beginning everything has been equalized, equilibrium shall exist between the pairs of plates of the one and the other side. Frequently, previous to the addition of the acid in the cells of the one system, a certain side, in which the pairs have a somewhat more powerful electro-motive state, has an ascendancy, although if proper precautions have been taken this is very weak. In this case, however, the experiment can be rendered available for our purpose quite as well by adding at present the acid to that series of cells which appears to be the weakest. According to the chemical theory, the divergence of the multiplier should now quickly be reversed, instead of which it increases in the very direction which it previously had.

I will add yet the following variation of this experiment, connected with a measurement.

On one side five pairs of zinc and copper plates were arranged in a *couronne des tasses*, on the other five pairs of zinc and tin plates. The vessels of the first system were now filled only a third part as high as those of the second, the first with common spring water, the latter with strongly acidulated water, which caused a very lively development of gas. The first five plates connected by themselves into a circuit gave

a current which, measured by a multiplier by means of the initial oscillations, equalled 1.41 (reckoning the action of the earth upon the needles of the multiplier = 1); the second system, forming a circuit by itself, produced a current, the force of which measured in the same manner was expressed by 44.7. When both circuits were combined into one with opposed direction of the current, it was evident that the pairs of zinc and copper plates still retained the ascendancy. The measure of the current resulting from the difference was 0.45.

5. Although the increase of chemical action in common circuits, not connected by too bad a conductor, indicates a very remarkable influence on the increase of their power, yet this favourable influence is lost in proportion to the increased opposition in the connecting conductor; thus, for instance, the power of the current of a circuit rose, by connexion with proportionally short and thick wire, by the addition of a certain quantity of acid, in the proportion of 1 : 191. (The plates were newly filed before the introduction of the acid, and the power, as in the first case, measured by the initial oscillations only.) As the same experiment was repeated, under quite the same circumstances, but by connecting the poles with a very long and thin wire which evinced about 7000 times the opposition of the former, the power rose by equal strength of the fluid only in the proportion of 1 : $1\frac{1}{3}$ (other examples of this kind are enumerated in my galvanic measurements). In the whole chemical theory I find no reason why the *proportion* of the increase of power should not be the same in both cases. With respect to the contact theory, according to which by increasing the strength of the fluid there is no increase in the quantity of electricity developed, but only a diminution of part of the opposition to the conduction existing in the circuit, no difficulty here presents itself, since the diminution of a part of the entire conductive opposition must lose influence in increasing the power of the circuit, in the ratio of its smaller amount in proportion to the other parts of the opposition of the circuit. If we lengthen the connecting wire more and more, it may at last be diminished so greatly that the opposition of the fluid becomes insensible.

III. *On the Development of Electricity by the Contact of Metals with Fluids.*

There still remains for me to notice some experiments which have been enumerated as bearing against the theory of contact, but which, properly speaking, merely prove (perhaps even appear merely to prove):

1. That it is not solely the contact of metals, or, generally, solid bodies with one another, which is capable of developing electricity.

2. That the excitation of electricity caused by the fluids, whether from their mutual contact, or from contact with solid bodies, does not follow exactly the same laws as that which arises from the mutual contact of solid bodies. The first is merely a generalization of the theory of contact, already made by Volta; the latter is no objection against it, since we do not know what influence the aggregate state has upon the still obscure process of the development of electricity. Experience certainly shows, undoubtedly, that fluids are not amenable to the same law of galvanic tension as solid bodies, or that if such is the case, secondary consequences resulting from the mobility of the particles, changes of metallic surface, or other circumstances, modify the result. The latter is my opinion, of which I have given a general explanation (although without paying sufficient attention to the changes of metals) in Biot's *Lehrbuch (Précis)*, Part III. p. 321. 372., and which, up to the present time, I have found no inclination to abandon. Then, almost all the facts recently published by Faraday appear to me to be for this reason much more important than they are represented to be by his own statements. Be this, however, as it may, (for I will force this view on no one) experiments in which we see electricity originate even without the contact of solid bodies, or so that this is of no influence, and, at the same time, thus observe the fluids acting a part different from that of solid bodies, cannot give proofs against the theory of contact. We must also consider, in this point of view, the following experiment by De la Rive, which I will relate, together with my own observations respecting it. It is found, with some little variation, in the *Recherches*, p. 62.

"To each end of a wooden cylinder, of from 10 to 12 centimetres in length, and 1 to 2 centimetres in diameter, I fastened a plate of zinc, which terminated outwards in a soldered brass point; taking at present the brass point of the one plate in my hand, I touched the condenser (also of brass) with the brass point of the other. According to the theory of contact, I ought to have obtained no sign of electrical activity, both the zinc and the brass plates lying opposite to each other, and being united by an insulated piece of wood which performed the office of conductor between both plates. Because, however, the one end of the wooden cylinder was moister than the other, I obtained signs of electricity, the nature of which bore a constant ratio to the chemical action, which was excited by the contact of the carefully brightened zinc with the moist

wood. These signs of electricity were negative, when I held between the fingers the brass point of that zinc plate, the other end of which was fastened in the less moist part of the wood. For the success of this experiment, it is necessary that the wood be somewhat moist; the moisture which it attracts from the air is perfectly sufficient; care must also be taken that the one end of the wood be kept drier than the other."

The truth of the result of the following, certainly interesting experiment, has been doubted, but it is certainly correct, as I have often convinced myself by a frequent and more careful repetition of it in the following easy manner.

Upon the zinc surface of a soldered zinc and copper plate were laid three or four equally large or somewhat larger leaves of air-dried writing paper, the top one moistened with distilled water, and upon this was placed, by the zinc surface, a second soldered zinc and copper plate; so that a system of the following order was formed: copper, zinc, dry paper, moist paper, zinc, copper. The zinc and copper were filed quite bright, and I had convinced myself that if I tried the system omitting the moist paper, at the copper condenser no development of electricity could be detected. When, however, I now applied the system with the inserted moist paper to the condenser in such a manner that the double plate lying on the moist paper came into connexion with it, while the other double plate stood in connexion with the earth by means of the fingers, either air-dried or sprinkled with distilled water, there was obtained, in accordance with De la Rive, a negative divergence, and a positive one on the contrary by reversion of the system. The same phenomena took place, if instead of distilled water I employed water acidulated by nitric acid for the purpose of moistening.

These experiments I have also varied and simplified in the following way:

Between two zinc plates, without copper, were arranged several layers of air-dried writing paper, and the one extreme layer on which lay the zinc plate was moistened with distilled or nitric-acidulate water. Sometimes the zinc plate situated on the dry, sometimes that situated on the moist layer of paper was put in connexion with a condenser of zinc. This experiment, still more simplified, may be performed as follows: let a zinc rod be wound round at one end with air-dried blotting paper; at the other, with blotting paper which has been moistened with spring or with distilled water. According now to the circumstance whether the dry or wet paper is discharged at the (brass) condenser, while at the same time the other paper is held with the fingers, do we obtain a posi-

tive or negative divergence. The discharge took place in both my cases on a leaf of writing paper moistened with distilled water, which was applied to the inferior plate of the condenser, while another leaf of moist paper covering the upper plate was touched with the fingers in order to make everything alike on both sides with respect to the condenser. The most simple form of the experiment might however be this; that a zinc condenser plate should be immediately touched with the moist fingers, which, as others have already observed, is sufficient to produce a negative shock. I do not say that this experiment is as yet explained agreeably to the contact theory, but as little could any one find in it a proof of the chemical theory. Moreover, this may be viewed in connexion with other much more important experiments which promise at least a partial explanation of it, of which, however, it is not now my intention to treat.

L. *Observations on some peculiar Properties acquired by Plates of Platina, which have been used as the Electrodes of a Voltaic Battery.* By GOLDING BIRD, M.D.F.L.S.F.G.S., &c., Lecturer on Natural Philosophy at Guy's Hospital.

THE influence of platina positive electrodes in effecting the combination of oxygen and hydrogen are well known to philosophers; some phenomena which may be probably referred to the same class have lately fallen under my notice, and are interesting from their appearing to prove that metals which have served as electrodes retain a polar state long after connexion with the battery is broken.

It was stated some time ago in a philosophical journal that when the platina plates of the ordinary apparatus used for exhibiting the decomposition of water by voltaic electricity on the lecture table, were placed in conducting communication with a piece of zinc immersed in the acidulated water with which the apparatus was filled, the hydrogen evolved at the surface of one plate was twice the volume of that given off at the other. The author of this statement added that he was unable to give any explanation of the fact, nor did he offer any remarks upon it. This phenomenon appeared to be of sufficient interest to deserve a more extended examination, and I have had the pleasure of observing several curious facts in connexion with it.

Exp. 1. A glass basin was furnished with two equal sized plates of platina passing through its bottom 1·5 inches apart, each connected by copper wires to a brass cup for holding

mercury; these cups, and the platina plates connected with them, may be called respectively A and B. The basin was filled with dilute sulphuric acid, and a tube full of the same fluid inverted over each plate of platina; a rod of amalgamated zinc, to one end of which was soldered two thin copper wires, was immersed in the contents of the basin, and the ends of the wires dipped into the cups of mercury A and B, by which the rod became metallically connected with the platina plates. Rapid decomposition of water instantly commenced, bubbles of hydrogen being evolved from the platina surfaces: in five minutes the tubes were examined, and instead of the gas collected being in equal volume in either tube, as would *a priori* be expected, I found, as stated by the original observer, that one contained nearly twice as much as the other.

Upon reflecting on this experiment, I suspected that as the apparatus had been employed as a volta-electrometer a short time previously, the platina plates might have assumed and retained some peculiar state from their connexion with the battery.

Exp. 2. The basin filled with the dilute acid was connected with a battery of six alternations of zinc and copper, separated by jars of porous earthenware and excited by sulphate of copper and sulphate of soda; the cup A was connected with the negative, and B with the positive wire: decomposition of water was allowed to proceed for a few minutes, contact was then broken with the battery; the tubes filled with diluted acid were inverted over the respective plates and the amalgamated zinc immersed in the acid, its wires dipping into A and B. Hydrogen was copiously evolved at the surfaces of the platina, and in 10 minutes the zinc was removed.

In the tube over the plate A was collected 1· inch hydrogen.

B ————— 2·15 —————

During the evolution of gas, the difference in the appearance of the bubbles from the two plates was remarkable: those from the plate B were large, rose rapidly to the top of the tube, and were given off from isolated points of the electrode; whilst those from A were small, rose much slower in the tube, and were given off from every part of the plate, resembling the bubbles of oxygen evolved in the voltaic decomposition of water. It is remarkable that the platina plate (A), which when in connexion with the battery gave off the greatest volume of gas, (hydrogen) now evolved the smallest, and that (B), which had evolved the smallest (oxygen), now gave off twice the volume of the other. It is needless to state that the gas in both tubes was hydrogen.

Exp. 3. The apparatus was refilled, again connected with

the battery, the plate A to the positive and B to the negative for a few minutes; the connexions being broken, the zinc rod was immersed as in the last experiment, its wires dipping into the cups A and B; hydrogen was again evolved and collected in the tubes.

In that over the plate A was found 2·1

————— B ————— 1·0

These experiments clearly pointed out, that the cause of the difference in volume of evolved hydrogen was to be sought in some change produced by connexion with the battery; what that change consisted in was less obvious. I suspected that a polar state might probably have been communicated to the electrodes by the passage of the battery current.

Exp. 4. The cups A and B were again connected with the battery, A with the negative, B with the positive wire for a few minutes; the connexions were then broken, and a galvanometer of very delicate construction connected with the cups. The needles instantly deviated with considerable velocity to 90° ; the connexion was broken, and in 1 minute renewed with the galvanometer; the needles deviated to the same amount but with less velocity. In 3 minutes, contact being again made, a deviation of 85° ensued, and in 15 minutes the experiment was repeated and a deviation of only 4° took place.

Exp. 5. The cup A was connected with the positive and B with the negative side of the battery during 6 minutes, and then the connexion was broken. On dipping the galvanometer wires into A and B, deviation of the needles to 90° ensued as in the last experiment, but *in the opposite direction*.

In both experiments, that platina plate which had been in connexion with the negative side of the battery and had then evolved hydrogen, appeared to be positive, deviating the galvanometer needles in the same direction as a piece of zinc.

The galvanometer made use of was extremely sensible, with light and nearly astatic needles suspended by a few fibres of unspun silk: the coil consisted of about 600 convolutions of well annealed and insulated copper wire wound on a frame on Nervander's construction. The frame being moveable on its axis could be brought into any position with regard to the needles.

The temporary polar condition assumed by platina electrodes after connexion with the battery has been before pointed out by De la Rive and other physicists, a rationale has also been proposed for it by Becquerel (*Traité de l'Électricité*, &c. 3. 109). This philosopher has only mentioned the case when the electrodes have been immersed in a saline solution, and after the contact with the battery is broken, con-

nexion made with the galvanometer; the deviation of the needles produced he attributes to minute films of acid and alkali adhering to the electrodes, combining to re-form the salt electrolyzed by the previous passage of the battery current, and thus give rise to a weak secondary current in a direction the reverse of that of the battery. This explanation will, I think, hardly apply in the above experiments, as the electrolyte was dilute sulphuric acid, and no other elements present, separable by the electricity, whose reunion could excite a current except oxygen and hydrogen; and even if the polar state be attributed to the previous decomposition of infinitely minute quantities of saline matter *accidentally* present, still it would require more satisfactory evidence than we yet possess to show that the recombination of the separated elements of the salts would develop sufficient electricity to act with such energy on the galvanometer, particularly as the late experiments of Dr. Mohr, of Coblenz, have rendered it very doubtful whether electric currents are ever set in motion during the combination of an alkali with an acid except in the solitary case of nitric acid, and which then may, perhaps, be attributed to a very different cause*.

For the purpose of discovering some clue to the cause of this curious phenomenon, the following experiments were performed.

Exp. 6. The cup A was connected to the negative and B to the positive wire of the battery; the connexion being removed, the deviation produced by connecting A and B to the galvanometer was ascertained to be 90° : the glass basin was then emptied of its acid, and set aside for 48 hours; at the expiration of that time it was refilled with dilute sulphuric acid and connected again with the galvanometer; the needles deviated to 6° in the same direction as in Exp. 5.

Exp. 7. The galvanometer wires being removed, the amalgamated zinc rod was immersed in the acid contained in the basin, its two wires dipping into the cups A and B; gas was as before evolved from both platina plates; this was collected in separate tubes and measured.

| | |
|---|---------|
| | inches. |
| In the tube over the plate A was found 1.95 gas = 1.0 | |
| B ————— 2.20 — = 1.13 | |

Exp. 8. The cups A and B were connected with the battery as in Exp. 6, for five minutes; the apparatus was removed from the battery, and set aside for 48 hours full of the acid. On

* "Ueber Becquerel's Einfache Kette, deren Strom aus dem Verbindung von Säure und Alkali entstehen soll."—Von Dr. Mohr, *Poggendorf, Annalen*, xlii. p. 76.

then making a communication with the galvanometer, the needles moved to 30° .

Exp. 9. The galvanometer being removed, the zinc rod was immersed as in Exp. 7, and the hydrogen given off collected in separate tubes.

In the tube over plate A was found 4.20 inches = 1.0

————— B ————— 5.15 ——— = 1.23

Exp. 10. The cup A was connected to the negative and B to the positive side of the battery: decomposition of water was allowed to proceed for 5 minutes; the wires were then removed, the basin emptied of its contents, and repeatedly rinsed out with cold water. It was then refilled with dilute sulphuric acid, the rod of amalgamated zinc immersed, its wires dipping into the cups A and B: the hydrogen evolved was collected and measured.

In the tube over the plate A was found 1.8 inches = 1.0

————— B ————— 1.7 ——— = 1.3

Exp. 11. The basin of the apparatus was filled with the dilute acid, connected with the battery for 20 minutes, emptied, washed out with cold water, refilled with acid and connected with the galvanometer; again placed in communication with the battery for the same time, emptied, and washed out with boiling water: this was repeated, the plates of platina being washed with a solution of potass, and in a fourth experiment with nitric acid; the basin being always refilled with the same dilute sulphuric acid before being connected with the galvanometer. The deviations produced were—

After washing the electrodes with cold water 80°

————— boiling water 73°

————— solution potass ... 45°

————— nitric acid 4°

Exp. 12. The basin of the apparatus was filled with a solution of pure potass, sp. gr. 1.064 and connected with the battery during 5 minutes as in the preceding experiments. On then dipping the galvanometer wires into the cups A and B, the needles deviated to 45° . The basin was then emptied, washed out with cold water, and refilled with the alkaline fluid: on making connexion with the galvanometer, the needles only indicated 5° .

Exp. 13. The basin filled with the solution of potass was again connected with the battery as in the last experiment; it was then emptied, filled with dilute sulphuric acid and connected with the galvanometer: the needles instantly deviated to 90° with considerable velocity.

In all these experiments the *direction* of the deviations was

constant; the plate of platina, which had during connexion with the battery evolved hydrogen, always being positive, and acted on the galvanometer like a piece of zinc. No appreciable difference in the surfaces of the platina plates with regard to lustre, &c., was observed. From the results of these experiments, we are, I think, hardly warranted in supposing with Becquerel, that the polar state of the plates arose from a film of acid and alkali adhering to them, although in other cases that this may be the cause of the polarized condition of the electrodes no one who has read the remarks of this distinguished philosopher on the battery of constant currents can doubt (*Traité*, 5. 219.), and perhaps Exp. 13 may admit of an explanation of this kind. The fact of the polar state being to a great extent retained after washing the plates with water, alkalies, exposure to the action of acids for 48 hours, &c., is certainly opposed to the hypothesis of Becquerel, although I am aware that films of sulphuric acid cannot be entirely removed from plates of platina by washing, as Dr. Faraday has satisfactorily shown. The *direction* of the secondary currents, being opposed to that of the battery currents, appeared at first to connect these phænomena with those of electro-dynamic induction. The fact of the secondary current being continuous for some time and gradually disappearing, instead of being only of momentary duration, might be supposed to arise from the badly conducting nature of the fluid connecting the plates of platina; this hypothesis, suggested by some experiments, was at once refuted by the secondary currents being detected by the galvanometer, after emptying the glass basin and filling it with *fresh* dilute acid.

The cause of the evolution of a double volume of hydrogen from the positive plate is less mysterious, this being evidently analogous to the phænomena observed by Dr. Faraday in his researches on the action of platina plates in effecting the combination of oxygen and hydrogen gases; he found that the plates which had served for positive electrodes were very energetic in producing this combination, whilst the negative electrode was quite inactive, and this difference he attributed to the surface of the positive plate being more polished, and freer from foreign matter than the negative. This view was confirmed by the fact of the negative plates becoming active by mechanically cleaning them. In the same manner I consider that the positive plates when connected metallically with the zinc rod, evolved a larger volume of hydrogen in a given time than the negative, because their surfaces were perfectly free from accidental tarnish, which although not readily appreciable by the eye, yet materially affects their electrical

condition. On this hypothesis the results of the other experiments may be explained: thus the volumes of gas evolved from the plates became more equal after the platina had been exposed to the action of those agents which would tend to remove superficial impurities (Exp. 9, 10, &c.); and the intensity of action on the galvanometer appeared to be connected with a similar state (Exp. 11,) as the electric condition of the plates became nearly equal, after washing them with nitric acid, a deviation of only 4° taking place. What the substance is, whose *invisible* deposition on the negative plate causes this current, is conjectural; that it is not an alkali, as Becquerel has suggested, is I think proved by the positive electric condition not being removed by dilute sulphuric acid, as well as from the almost infinitesimally minute quantity of saline matter present in the electrolyte employed in my experiments. That a very small quantity of foreign matter is sufficient to produce a very considerable action on the galvanometer is proved, by using for these experiments the dilute acid that has been previously employed for the generation of gas from the amalgamated rod, instead of fresh portions of that fluid; the minute and imperceptible portion of zinc reduced on the negative electrode producing most intense action on a very delicate galvanometer.

The negative plate I found became capable of evolving as large a volume of gas as the positive electrode, by scouring it with nitric acid and fine sand, evidently proving its former comparatively inactive state to depend upon some superficial deposition; the same result was obtained by employing both plates simultaneously as positive electrodes.

Exp. 14. Two copper wires were for this purpose screwed upon the last copper plate of the battery, an end of each dipping into one of the cups A and B, the wire from the last zinc plate dipping into the dilute acid in the basin; under these circumstances *both* platina plates evolved oxygen, and were of course positive electrodes. On then breaking battery connexion and dipping the zinc rod into the acid, its wires being immersed into the cups A and B, equal volumes of gas were evolved, *ceteris paribus*, from both plates.

From the results of this investigation I think I am justified in referring all the phenomena observed to the peculiar condition of surface assumed by the platina plate that has served as the negative electrode, its inactive state *quoad* the evolution of hydrogen, and its active state *quoad* the galvanometric current, depending upon one and the same cause, viz. the deposition of infinitesimally minute portions of foreign matter on it, which serves to diminish the rapidity of the evolution of

gas at its surface, whilst, in consequence of its being attacked and ultimately dissolved by acids, it causes the platina plate covered by it to assume a positive state, and act as such upon a delicate galvanometer. That the substance deposited is not an alkali, or anything very soluble in water or acids, has been, I think, quite proved; perhaps it may differ according to the nature of the electrolyte employed.

22, Wilmington Square, Sept. 29, 1838.

LI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 310.]

June 6th.—A paper was first read, "On Spirolinites in Chalk and Chalk-flints;" by the Marquis of Northampton, F.G.S.

The fossils described in this memoir were chiefly found in the flints of Sussex; and though above two hundred specimens, more or less perfect, have been discovered by the author in that county, he has very rarely met with the genus elsewhere. They have been also found in the same county by the Rev. G. Smith and Mr. Walter Mantell. They occur more frequently in the grey than the black flints; and are sometimes accompanied by innumerable minute fragments of other organic bodies. The size of the best-preserved specimens, including the prolongation, is about one-third of an inch; and the number of chambers or divisions varies in the same species.

Of the six species described in the paper, one had been previously named, by Mr. Mantell, *Spirolinites Comptoni*, and the other five have been named by the Marquis of Northampton, *S. Murchisoni*, *S. Stokesii*, *S. Lyellii*, *S. Mantellii*, and *S. Bucklandii*; the author, however, stated that it is exceedingly difficult to determine satisfactorily specific differences, especially in such minute fossils, imbedded in flint, and exposed only by accidental fracture.

A communication was next read, entitled, "A Note to accompany Specimens of Quicksilver Ore from the mine San Onofre, near the town of El Doctor, Mexico." By John Taylor, Esq., Treas. G.S.

Though quicksilver had been known for some time to exist in Mexico, yet few attempts had been made to ascertain to what extent, till the increased prices and demand induced the directors of the Real del Monte and Bolaños Mining Companies to have researches made. Ores of that metal have, in consequence, been discovered and traced in districts very remote from each other; but Mr. Taylor confined his remarks to the locality at which the specimens presented to the Society were obtained.

The ores of San Onofre are chiefly cinnabar, partly hepatic, but native quicksilver occurs, and native calomel. They are stated to exist in a regular vein, two or three yards in width, the gangue consisting of carbonate of lime, with sulphate of barytes, and a small portion of silex. It traverses a limestone hill of considerable height, and appears to have been worked in former times.

Extracts were also read from a letter addressed to John Taylor, Esq., Treas. G.S., by Mr. Frederick Edmonds, explanatory of some specimens of obsidian from the mountain of Real del Monte, Mexico, collected by Mr. Frederick Edmonds, and presented to the Society by Mr. Taylor.

About half a mile from the ancient obsidian mines is the Cerro de las Navajas, in which several passages may be observed from an earthy felspathic rock to perfect obsidian. Although no good section is exposed, the obsidian is stated to occur in irregular beds, chiefly vertical, and Mr. Edmonds is of opinion, that it has been derived from the fusion of the felspathic rock under pressure. The collection presented to the Society's museum, contained a specimen of calcareous tuff from the thermal springs of El Grande.

A notice was next read of a specimen of the Oar's Rock, nine miles south of Little Hampton, Sussex, by Roderick Impey Murchison, Esq., V.P.G.S.

The specimen consists of a calcareous grit, bearing no resemblance to the Bognor rock, or other beds above the chalk, but agrees with some of the beds in the green sands below that formation, or with the Portland beds. Mr. Murchison's principal object in laying this notice before the Society was to point out that the Oar's Rock is between the parallels of disturbance which traverse the Wealden of Sussex on the north, and the Isle of Wight on the south, the intermediate space being also traversed by the protruded chalk-outliers of Portsdown Hill, north of Portsmouth, and High-down Hill near Worthing, described by Mr. Martin*. Mr. Murchison, therefore, inferred that the Oar's Rock may indicate the protrusion of strata at that point. He noticed likewise the earthquakes so frequently felt at Chichester, which is situated intermediate between Portsdown Hill and Highdown Hill.

A paper was then read, "On the discovery of Fossil Fishes in the Bagshot sands at Goldworth Hill, 4 miles north of Guildford;" by the Rev. William Buckland, D.D., F.G.S.

These remains were recently obtained in cutting through the summit of Goldworth Hill, on the line of the London and Southampton Railway, and their preservation is due to Mr. Sibthorpe of Guildford. The only organic bodies previously discovered in the Bagshot sand are the casts of shells found near Chobham Park, and noticed in Mr. Warburton's account of the formation.† The fossils described by Dr. Buckland, were obtained from a bed of greenish sand, the sixth from the top of the section; and they consist of a few imperfect casts of marine shells, and the remains of marine fishes. The most numerous among the latter are the teeth of sharks, and the palates and teeth of rays, similar to those which abound in the London clay. One large tooth of a saw-fish, procured from the same bed, affords the first well-authenticated example of the discovery, in England, of the genus *Pristis*. In addition to these remains, three

* Geological Memoir on part of Western Sussex, p. 93, 1828. [Or Phil. Mag. and Annals, N. S., vol. iv. p. 46.]

† Geological Transactions, 2nd Series, vol. i. p. 48.

forms of cartilaginous fishes, and a few vertebræ of bony fishes, resembling those procured in Sheppey, have been found, also three new genera of fishes. For the latter Dr. Buckland proposes the names of *Edaphodon*, *Passalodon*, and *Ameibodon*. In the first genus the structure of the teeth is nearly allied to that of the broad and flat palates so common in the oolitic and carboniferous limestones. There were three of these teeth in the upper jaw, and three in the lower on each side, and they were disposed so as to form a pavement, arming the interior of the mouth with powerful instruments for crushing shells. Their surface is pitted with minute depressions, disposed in nearly the same manner as in the genus *Psammodus*. The body of each tooth also, as in *Psammodus*, is composed of hollow, long, cylindrical columns, placed nearly at right angles to the working surface. The bones to which the teeth are attached, are not granulated as in cartilaginous fishes; but are cancellated after the manner of bony fishes. The mode of insertion of the teeth in both jaws offers many peculiarities, as they are placed in nearly horizontal, long sockets, and maintained by continual growth from the posterior extremity of each tooth. Details were given of the genera *Passalodon* and *Ameibodon*; and it was stated that *Edaphodon* and *Passalodon* offer combinations of the characters of cartilaginous and bony fishes. In the stratum in which these remains were found, Mr. Sibthorpe has also discovered portions of the carapace of an *Emys*, resembling that of the London clay; and Sir Philip Egerton possesses a fragment of a tooth found at Sheppey resembling the teeth of the *Edaphodon* of Goldworth Hill. From the agreement, therefore, in the fossils of that locality with those of the London clay, Mr. Lyell's opinion, that the Bagshot sand was deposited during the eocene period, has received additional support.

A notice, by Dr. Buckland, was afterwards read "On the discovery of a fossil wing of a Neuropterous Insect in the Stonesfield slate."

The elytra of several species of coleopterous insects have been, for some time, known to occur in the Stonesfield slate, but Dr. Buckland believes that this is the first discovery of any remains of *Neuroptera*. The wings of *Libellula* are not unfrequent at Solenhofen; and a neuropterous wing, resembling that of a *Corydalid*, has been discovered by Mr. Mantell in an ironstone nodule from Coalbrook Dale. To the notice was appended a description of the wing by Mr. Westwood, from which the following is an extract:—"I have compared the fossil insect wing with the various genera of neuropterous insects, both indigenous and exotic, but it agrees with none of them. I apprehend there can be no doubt that it belonged to a tetrapterous insect, and to the order *Neuroptera*."

Dr. Buckland proposes to call the fossil insect *Hemerobioides giganteus*, from its being more nearly allied to the recent genus *Hemerobius* than to any other at present known.

The last paper read was on some species of Orthocerata; by Charles Stokes, Esq., F.G.S.

In Dr. Bigsby's paper on the geography and geology of Lake

Huron* some *Orthocerata* of peculiar forms are described; but since the publication of that memoir, Mr. Stokes has received many other specimens, collected during the expeditions of Sir Edward Parry, Sir John Franklin, the late Capt. Lyon, and Capt. Back, and by Capt. Bayfield during his survey of the lakes and the river St. Lawrence. The object of the present communication is to describe some species indicating generic separations among the *Orthocerata*, and to call attention to certain considerations respecting the relations of the shells to the animals to which they belonged.

The first generic distinction adopted by Mr. Stokes consists in a large siphuncule, much dilated in each chamber, and contracted at the parts where the septa are attached to it. Within the siphuncule is a continuous tube which appears to have been capable of expansion or contraction, and is furnished with radii in verticillations, which connect the tube with the walls of the siphon. For this genus he retains the name of *Actinoceras*, given by Bronn† to figures 1—3, Pl. 25, vol. i. N.S. of the Geological Transactions. Four species have been established by Mr. Stokes; namely, *Actinoceras Lyonii* from Igloolik and Ooglit; *A. Bigsbii* from Thessalon Island, in Lake Huron; *A. Richardsonii* from Lake Winnipeg; and *A. Simmsii* from Castle Espie, in the County of Down, Ireland.

The character of the second genus is a siphunculus, similar in external form to the preceding, but the inner part is divided into portions corresponding in number with the chambers, and deeply indented in the middle, where the septa of the shell are attached to them; so that one half of each division of the siphuncule is in one chamber, and the other half in the next chamber. The opening or interior passage is comparatively small, and the inner and outer walls of these divided portions, which are separated by a considerable space, are beautifully curved. From the resemblance of the siphuncule to a row of beads, Mr. Stokes proposes to call the genus *Ormoceras*. Three species were described, all of them obtained from Drummond Island in Lake Huron; *Ormoceras Bayfieldii*, *O. Backii*, and *O. Whitei*.

Among the fossils noticed in Dr. Bigsby's paper, were several which Mr. Stokes then considered to be corals, from the internal plates, and to which he gave the generic name of *Huronia*‡. He has, however, since discovered, that they do not possess the peculiar central structure, exhibited by the greater number of the lamelliferous corals; but that they have a continuous central opening; and, from the examination of other specimens, he now considers that these bodies are the siphuncules of true *Orthocerata*. He proposes, nevertheless, to retain the generic name of *Huronia*; and has called the only species, yet found with traces of the septa, *Huronia Portlockii*.

Mr. Stokes then offered some remarks "respecting the relations of the shell to the animal to which it belonged." From the si-

* Geological Transactions, 2nd Series, vol. i. p. 195 *et seq.* Pls. 25 and 26.

† *Lethæa geognostica*, vol. i. p. 98. tab. i. fig. 8. 1835.

‡ Geological Transactions, 2nd Series, vol. i. p. 202, Pl. 28.

phuncule being so often preserved without the external portion of the shell, and the latter, including the septa, being, when retained, extremely thin, he is of opinion that the shell must have been enveloped in the animal to protect it from injury. This view of the relation of the shell to the soft portions of the creature, he is of opinion, receives confirmation from his having observed only one instance of a parasitic body being attached to *Orthoceras*. In this instance, indeed, there are evidences that the parasite, a coral, must have been formed after the death of the animal of the shell (*Actinoceras Simmsii*), for the specimen exhibits in some parts, layers of earthy matter, alternating with layers of the coral; and, therefore, the growth of the latter must have been more than once interrupted by the deposition of sediment. The memoir concluded with some remarks on a peculiar subdivision in the septa towards the outer wall of one side of the chambers, noticed by the author in several specimens of *Ormoceras*, in an *Orthoceras* from Russia, and in *Lituities*; but he refrained from offering any opinion, whether this character is common to the family of *Orthoceratidae*, or is confined to certain genera only.

This being the last evening of the Session, the Society adjourned to Wednesday, November the 7th.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

At the Anniversary Meeting held on the 28th of September, 1838, the President, Davies Gilbert, Esq., D.C.L., F.R.S., in the Chair, the Twenty-fifth Annual Report of the Council was read.

After noticing Her Majesty's reception of the address of congratulation and condolence from the Society, and her consent to be its Patroness, the Council proceed as follows:

"The Map (of what may perhaps not improperly be termed the *surface* Geology) of Cornwall, executed by Mr. De la Beche, and the illustrative Memoir by which it is to be accompanied,—anticipated in the last Report,—have not yet appeared.

"Unforeseen circumstances, chiefly on the part of the printer, have delayed the appearance of the Fifth Volume of the Society's Transactions; but a considerable portion of it, embracing a large part of Mr. Henwood's Memoir on the Mines of the County, on which he has been more or less occupied for twelve years past, is now on the table; and, the surveys being all completed, it will be published in the early part of the ensuing year.

"Dr. Boase has been compelled to delay his Memoir on the diluvium and other transported matter of Cornwall; but it is only deferred, not withdrawn, and will occupy a prominent place in the next volume of Transactions.

"The Donations to the Museum and Library, during the past year, have not been very numerous, but in value they have not been often exceeded: among others, the Council desire to particularize the extensive collection of Organic Remains from the Chalk of Sussex, presented by Mrs. Davies Gilbert; an excellent series of the magnesian limestone from Northumberland, by W. Mansel Tweedy, Esq.;

a beautiful suite of Madrepor limestone from the neighbourhood of Torquay, by John Edward Lee, Esq.; a second contribution illustrative of the geology of India, by the Rev. George Pigott (late librarian to the Society); some splendid coal fossils from Radstock, by Henry Pendarves Tremenhoe, Esq.; the Rev. Canon Rogers's valuable suite of specimens from the Serpentine of Pennare Point, in this county; an admirable series of the rocks and minerals of the Wherry Mine, by Richard Pearce, Esq.; and some very beautiful minerals from Cuba, by John Garby, Esq.

"The Council also notice with peculiar satisfaction the interest shown in the advancement of the Society, by the practical miners of the county; and they refer particularly to the valuable donations of Messrs. William Francis of the Consolidated Mines, John Williams of the Virtuous Lady Mine, and John Lyle of the Carn Brea Mines; Mr. Peach has also, in the present year, made considerable additions to the collection of organic remains which he presented at the last anniversary.

"The establishment of an Academy for Instruction in the Arts and Sciences, connected with mining in Cornwall, has been a long-cherished object of the Society; but it is obvious that such an institution is not likely to be formed, until the mining interest in general shall be convinced of its utility and necessity, and shall express their opinion in its favour. The Council rejoice in the belief that this conviction is increasing; and that it will not be long before, either by the establishment of a kind of experimental college, or by the periodical visits of the principal professors of the arts and sciences connected with mining, the reproach which has been long and deservedly applied to Cornwall, or rather to England, that, with the richest and most instructive mining field in Europe, it is behind almost every other nation in furnishing scientific information to its mining population in general, and in providing the means of leading those on whom the management of the mines principally depends, into the higher parts of the sciences which bear upon the theory and the practice of mining,—will be entirely wiped away.

"During the meeting of the British Association at Newcastle, a committee was appointed for the purpose of assisting different mining districts in an united effort to originate a school for instruction in the arts and sciences, connected with mining, on an enlarged scale; and the council embrace the present opportunity of calling on the members, and all who are in any way connected with the mining interest of this County, and the interests of science generally, to use their best exertions to promote so important an object."

The following papers have been read since the last Report. Notes on the Serpentine of Pennare Point. By the Rev. Canon Rogers, A.M., Member of the Society. Notice of a raised beach immediately under a part of the Town of Penzance. By Joseph Carne, Esq., F.R.S., &c. On the joints in the Chalk of Beachy-head. By John S. Enys, Esq., Member of the Society. On the metallic copper found in the turbaries near Dôlgelley, Merioneth. By W. J. Henwood, F.G.S., London and Paris, H.M., Assay Master

of Tin in the Duchy of Cornwall, Secretary to the Society, and Curator of the Museum. On the assaying of copper ores by means of Galvanism. By Martyn J. Roberts, Esq., Corresponding Member of the Society. On the expansive action of steam in the cylinders of the Cornish Pumping Engines. By W. J. Henwood, F.G.S., Hon. M.Y.P.S., &c. On the rocks associated with the metalliferous formations of the Wherry Mine. By Richard Pearce, Esq., Member of the Society. On the organic remains in the slates of Paraver, Parr, Polkerris, Fowey, and Polruan. By C. W. Peach, Esq., Associate of the Society. On the raised Beaches of Portscathoe. By John S. Enys, Esq., Member of the Society. On a section of the cliffs on the north coast of Cornwall, from Cligga Point to Lower Saint Columb Porth. By W. J. Henwood, F.G.S., Corresponding Member of the Plymouth Institution. On some improved methods of blasting rocks. By Martyn J. Roberts, Esq. An account of the Quantity of Tin produced in Cornwall and Devon, in the year ending with the Midsummer Quarter, 1838. By Joseph Carne, Esq., F.R. & G.S., M.R.I.A., &c. An account of the Quantity of Copper produced in Great Britain and Ireland, in the year ending 30th June, 1838. By Alfred Jenkyn, Esq.

The following Gentlemen have been elected since the last Report. *Corresponding Members.*—John Edward Lee, Esq., of Hull. Martyn J. Roberts, Esq. *Ordinary Members.*—Vincent Pryce, Esq., of Redruth. Frederick Rogers, Esq. The Rev. George Boraston, of Kenegie. The Rev. H. Comyn, of Sancreed. Edwin Ley, Esq. John Vigurs, Esq. *Associates.*—Captain John Lyle, of Camborne. Captain James Evans, of St. Agnes. John Garby, Esq., of Redruth.

Officers and Council for the present Year. President.—Davies Gilbert, Esq., D.C.L., V.P.R.S., &c., &c. *Vice-Presidents.*—John Basset, Esq. Day P. Le Grice, Esq. E. W. W. Pendarves, Esq., M.P., F.R.S., &c. Henry Woolcombe, Esq., F.A.S., &c. *Secretary and Curator.*—W. J. Henwood, F.G.S., London and Paris. *Treasurer.*—Joseph Carne, F.R.S., F.G.S., &c. *Librarian.*—Richard Hocking, Esq. *Council.*—Capt. Davies, R.N. Mr. James Flamank, Charles Fox, Esq. William Gibbins, Esq. Rev. H. E. Graham. Edwin Ley, Esq. Richard Pearce, Esq. Major Robyns. W. Mansel Tweedy, Esq., F.H.S., &c. John Vigurs, Esq. L. R. Willan, M.D. Wm. Williams, Esq.

LII. *Intelligence and Miscellaneous Articles.*

REAGENT FOR THE DETECTION OF SULPHUROUS ACID IN THE HYDROCHLORIC ACID OF COMMERCE. BY GIRARDIN.

THE hydrochloric acid of commerce often contains sulphurous acid, especially when prepared on the large scale in the alkali manufactories: this contamination cannot be detected by the odour, but in the protochloride of tin we have an excellent test for its presence. For this purpose, crystals of the chloride should be dissolved in the suspected acid, which if free from sulphurous acid will dissolve the chloride, with the production of a very insignificant troubling. If

this acid is present however, the hydrochloric acid becomes rapidly brown, by repose depositing a precipitate of that colour, consisting of an intermediate sulphuret of tin, of proto-sulphuret and of chloride of tin insoluble in muriatic acid. This reaction depends upon a portion of the tin becoming oxidated at the expense of the sulphurous acid, another portion uniting with the sulphur, while the oxide of tin thus formed dissolves in the excess of muriatic acid to form an insoluble chloride.—*Journ. für Pract. Chemie*, 6. 81.

PROCESSES FOR PREPARING LITHIA. BY FUCHS.

1. Lepidolite is to be ignited during two hours with $1\frac{1}{2}$ or 2 parts of quicklime, and the mass left during two or three months exposed to the air, it being kept constantly moist with water. The whole is then to be powdered and treated with water, which dissolves out potassa and lithia; the solution is then saturated by a current of carbonic acid gas and evaporated to dryness. The residue, which consists of the carbonates of potassa and lithia, is digested with water containing a little alcohol, which dissolves the carbonate of potassa and leaves the carbonate of lithia.

2. Seven parts of lepidolite are ignited with 2 of calcined vitriol, taking care that the mixture never melts. The mass when cold is powdered and boiled with water, which dissolves out the sulphates of lithia and potassa: the solution is then evaporated to dryness, and the salt of lithia dissolved out by water, to which a little alcohol has been previously added.—*Jahres-Bericht der Phys. Wissenschaften* 16. 97.

NEW DOUBLE SALT OF ZINC AND POTASSIUM. BY ANTHON.

When a solution of iodide of potassium is mixed with one of nitrate of zinc, a yellowish crystalline deposit of iodide of zinc falls, which dissolves on the addition of an excess of iodide of potassium, and by evaporation crystallizes in colourless octohedral or rhombic crystals, which are not altered by exposure to the air, and contain iodine, nitric acid, zinc, and potassa. By exposure to heat this salt evolves iodine, then nitrous acid, oxide of zinc and potassa being left; it is readily soluble in water, insoluble in alcohol; acids precipitate iodine from its solutions, tartaric acid separating bitartrate of potassa.—*Buchner's Repertorium*, 1. 115.

REAGENT FOR NITRIC ACID AND NITROGEN. BY DESBASSAYNS DE RICHEMONT.

The mode of detecting the presence of nitric acid proposed by De Richemont is exceedingly delicate, and depends upon the fact, that a mixture of a concentrated solution of protosulphate of iron and sulphuric acid becomes coloured rose-red by the addition of deutoxide of nitrogen (*Stickoxyd*), or even purple if the latter is present in larger proportion: the quantity of the nitric oxide necessary for the production of this tint is so small that an exceedingly minute portion may be detected by it. To detect the presence of nitric acid, add to a small quantity of sulphuric acid the solution to be examined, in

such a proportion that the quantity added should equal three-fourths the bulk of the acid. When the mixture has become cool, drop in a concentrated solution of protosulphate of iron, which if any nitric acid is present decomposes it, causing the evolution of nitric oxide which produces the rose-red or purple tint above alluded to. This mode of operating will allow us to detect 1 part of nitric acid in 24,000 of water.

For the detection of nitrogen gas, De Richemont directs the gas under examination to be mixed with from 3 to 6 times its volume of a mixture of oxygen and hydrogen (in equal vols.) and the whole detonated in a Eudiometer by the electric spark. The fluid that bedews the Eudiometer after the explosion is to be mixed with sulphuric acid, to which a few drops of protosulphate of iron in solution have been added: the fluid will assume the rose-red tint if the minutest portion of nitrogen is present. It is of course necessary to avoid any source of fallacy arising from the presence of atmospheric air in the oxygen and hydrogen employed. The action of sulphuric acid in producing this rose colour is so remarkable that when we have failed in detecting the presence of nitric acid by the ordinary process from its being present in too small a quantity to tinge the sulphate of iron *brown*, the addition of concentrated sulphuric acid will determine the production of the rose-red tint, if any nitric acid existed in the fluid under examination.—*Journ. für Pract. Chemie*, 5. 207.

FORMATE OF SODA, AS A REDUCING SUBSTANCE FOR METALLIC POISONS (ARSENIC).

Göbel has found that formate of soda furnishes the most ready means of reducing metallic poisons, not only when in the state of oxides but as sulphurets, and is therefore of extreme importance in researches connected with medico-legal inquiry. The substance to be examined is mixed with the formate and heated in the usual manner in a small glass tube, over the flame of a lamp; the arsenic if present of course sublimates: in this way Göbel has detected the presence of orpiment in the golden sulphuret of antimony when present only in the proportion of 1 part to 1000 of the antimonial sulphuret.—*Jahres-Bericht der Phys. Wissenschaften*, 16. 162.

ON THE TRANSPARENCY OF CARBON. BY DEGEN.

Degen has attempted to prove that charcoal in very thin layers possesses a very considerable degree of transparency, and appears of a yellowish-brown colour. To demonstrate this he placed a piece of pine charcoal on burning coals in a small furnace, so close that very little air could enter it, so that the piece of charcoal should undergo combustion in an atmosphere loaded with carbonic acid: under these circumstances the more porous portion burns, whilst the denser portion, consisting of an aggregation of small vessels, is left: the walls of these carefully examined under a microscope, appear translucent.

If a piece of glass covered with lac-varnish and sprinkled with charcoal powder is ignited in a crucible, so as to carbonise the varnish, the thin layer (of carbon?) left is transparent and yellowish brown.—*Poggendorff's Annalen*, 35. 468.

PREPARATION OF ARSENIURETTED HYDROGEN. BY VOGEL.

When two equivalents of zinc are melted and one equivalent of powdered metallic arsenic added, they combine after stirring the mixture, which without any fresh application of heat becomes suddenly red hot. A little arsenic volatilizes, which burns on the surface of the mass with a blue flame. If we take equal equivalents of the two metals, the same evolution of heat occurs, but not to so great an extent, and the mixture cools more quickly. Antimony fused with zinc does not produce this phenomenon. This alloy of arsenic and zinc when pulverized and treated with hydrochloric acid, evolves arseniuretted hydrogen free from any traces of precombined hydrogen gas, as it is completely absorbable by a solution of sulphate of copper; and therefore affords the readiest and most convenient mode of procuring this gas free from admixture.—*Journ. für Pract. Chemie*, 6. 343.

[The Editors are obliged to Dr. Golding Bird for the preceding notices from the German.]

TUNGSTATE OF TUNGSTEN AND POTASH.

M. Laurent prepared this salt by the method employed by M. Wöhler for the corresponding salt of soda. This tungstate crystallizes in small needles, whilst the soda salt is in small cubes.

M. Laurent states that the colour of this salt is extremely beautiful; it is of a deep coppery violet colour, and very brilliant when looked at in the sunshine; it very perfectly resembles sublimed indigo; and, what is remarkable, when burnished it becomes, like indigo, of a fine blue with coppery reflections. The corresponding salt of soda, whatever may be its colour, becomes blue by friction.—*An. de. Ch. et de Ph.* lxvii. 219.

ANALYSIS OF SERUM OF BLOOD DRAWN FROM A DIABETIC PATIENT. BY DR. G. O. REES.

| | |
|---|---------|
| Water | 908.50 |
| Albumen (yielding oxide of iron and phosphate of lime by incineration)..... | 80.35 |
| Fatty matters | 0.95 |
| Diabetic sugar | 1.80 |
| Animal extractive soluble in alcohol, and urea .. | 2.20 |
| Albuminate of soda | 0.80 |
| Alkaline chloride with trace of phosphate | } 4.40 |
| Alkaline carbonate, trace of sulphate, the results of incineration | |
| Loss | 1.00 |
| | 1000.00 |

Guy's Hospital Reports, Oct. 1838.

ANALYSIS OF THE LIQUOR AMNII.

Four specimens of this secretion recently examined by Dr. G. O. Rees, and described in the *Guy's Hospital Reports*, have yielded

precisely the same constituents. They were taken from different individuals at the same period of utero-gestation, viz. $7\frac{1}{2}$ months; their specific gravities were from 1.0086 to 1.007, and every specimen contained urea as an ingredient. The caseous matter, which is observed floating in the liquor, has been examined by Dr. R^{es}, and shown to contain cholesterine. The salts of the liquor amnii are the same as those of the blood; the alkaline phosphate and sulphate exist however as a mere trace. We subjoin the analysis of one specimen; the others differ from it merely in proportional constitution.

Liquor Amnii.

| | | |
|---|--|--------|
| Sp. grav. 1008, strongly alkaline, contained in 1000 parts. | | |
| Water | | 984.98 |
| Albumen (with traces of fatty matter) | | 1.80 |
| Extract soluble in water | Salts 2.80. | 6.02 |
| | Organic matter, principally albumen from the albuminate of soda, 3.22. | |
| Extract soluble in water and alcohol. | Salts 2.80. | 7.20 |
| | Organic matter, principally lactic acid; urea. | |

Guy's Hospital Reports, Oct. 1838.

STEAROPTEN OF TURPENTINE.

M. Brandes remarks that crystals have often been observed in rectified oil of turpentine, and these have been shown by several authors to be the stearopten of turpentine; and other chemists have obtained an acid resembling succinic acid. M. Brandes also obtained crystals from recently rectified oil of turpentine; they were collected on a filter, washed and dried; the quantity was small, amounting to only about 15 grains; they were transparent, had a vitreous lustre; some were dull; some were isolated, and others stellated; they were small, from half a line to three lines in length, and scarcely a quarter of a line thick; the form was a quadrilateral prism; with the assistance of a glass, the base appeared evidently to be rhombic, but differing but little from rectangular. The summit was dihedral. The crystals sunk in water.

A small quantity being heated in a globe, they readily sublimed in a fine filamentous mass, and in capillary crystals, some of which were more than an inch in length; when a larger quantity was heated, a portion liquefied during sublimation, and solidified on cooling into a small cake, which being again heated, again became fluid, and then sublimed without residue by the continued action of the heat.

The crystals are completely soluble in hot water, and dissolve also in alcohol and æther; the alcoholic solution may be diluted with several times its bulk of water, without becoming turbid; oil of turpentine had not so marked an action on the crystals when cold as alcohol and æther, but heat caused it to dissolve them completely, and the solution was not rendered turbid either by cooling

or by agitation with water. The crystals were not sensibly soluble in cold solution of ammonia or potash; when heated they were dissolved, and the solution was not rendered turbid by neutralizing the alkali; so that these substances appear to act only by means of their water of solution. The aqueous solution was neutral to test papers.

When rectified concentrated sulphuric acid is added to a small quantity of these crystals, a deep red-coloured solution is obtained, which, as has been remarked by Buchner and Hofner, has a smell resembling that of artificial musk; and, according to M. Brandes, an odour somewhat like naphtha, and also oil of fennel. Water produces a white cloud in the red solution, and yellowish white flocks gradually separate.

Hydrochloric acid when cold has no sensible action on the crystals, but when heated, a white cloud is produced; by boiling, the liquor bums with noise: a strong odour of oil of petroleum is developed. After cooling, the liquor, which was milky white, becomes clear, and a yellowish oil floats on its surface, which gradually hardens. Acetic acid, even cold, dissolves the crystals; the solution is not rendered turbid by water, nor is it altered by boiling. Nitric acid, when cold, dissolves the crystals slightly; but when heated they are gradually decomposed; a milky white cloud is formed in the acid, and drops of yellow oil are observable on its surface; it then yields a very distinct odour, resembling that of a mixture of oil of fennel, aniseed, and naphtha.

Increase of temperature occasions a more vivid action; the acid becomes yellow; nitric oxide is given out, with a smell more resembling camphor. The smallness of quantity of the crystals did not admit of their analysis; but the facts detailed agree with those previously described by M. Buchner, and prove that the crystals were the stearopten of turpentine.—*Journal de Pharmacie*, xxiv. 129.

METALLIC PECTATES.

The metallic pectates analysed by M. Regnault are those of silver and lead.

Pectate of Silver. The pectate of ammonia employed in forming this salt was prepared by dissolving pectic acid, which had been precipitated from solution by nitric instead of hydrochloric acid, in ammonia; this was done to be certain that no trace of chloride of silver might exist in the pectate.

The composition of pectate of silver is naturally variable, according to that of the pectate of ammonia which is employed in its preparation. The following were obtained:—

1st. Pectate of silver, prepared by adding a solution of nitrate of silver to a solution of pectate of ammonia, which had been boiled till it ceased to yield an ammoniacal smell. It consisted of—

Pectic acid 61·615

Oxide of silver 38·385—100·

In this compound the author is of opinion there was excess of acid, which he thinks might happen from the mode in which the pectate of ammonia was prepared.

Another pectate of silver was procured by pouring the solution of pectate of ammonia into that of nitrate of silver. This was composed of—

| | |
|-----------------------|------------|
| Pectic acid | 63·05 |
| Silver | 36·95—100· |

A second pectate of silver was formed by pouring a solution of nitrate of silver into one of pectate of ammonia evaporated in vacuo; this solution was neutral to test papers. It was formed of—

| | |
|-------------------------|-------------|
| Pectic acid | 59·612 |
| Oxide of silver | 40·388—100· |

A third pectate of silver, also procured from pectate of ammonia, evaporated in vacuo, gave—

| | |
|-------------------------|-------------|
| Pectic acid | 58·983 |
| Oxide of silver | 41·017—100· |

Lastly, nitrate of silver was added to solution of pectate of ammonia in which the alkali was in excess; as oxide of silver is soluble in ammonia, it was hoped that neutral pectate would be precipitated; but this was not the case; for the affinity of the ammonia for the oxide of silver counterbalanced that of the pectic acid.

The results of two experiments were—

| | | |
|---------------------------|-------|-------|
| Pectic acid | 61·52 | 63·32 |
| Oxide of silver | 38·48 | 36·68 |
| | <hr/> | <hr/> |
| | 100· | 100· |

All these analyses, the author concludes, indicate the same composition for pectic acid, but the capacity of saturation, he observes, remains undetermined.

[Taking, however, something near a mean of the results, and comparing them with those of the direct analysis, it appears that pectic acid is composed of—

| | | | |
|-----------------------------|----|----|-------|
| 8 eq. of hydrogen | 8 | or | 4·76 |
| 12 — carbon | 72 | | 42·85 |
| 11 — oxygen | 88 | | 52·39 |

| | | |
|------------|-----|------|
| Equivalent | 168 | 100· |
|------------|-----|------|

And supposing pectate of silver to be composed of one equivalent of acid and base, it will consist of—

| | | | |
|------------------------------|-----|----|-------|
| 1 eq. of pectic acid | 168 | or | 59·15 |
| 1 — oxide of silver | 116 | | 40·85 |

| | | | |
|------------|-----|------|-------|
| Equivalent | 284 | 100· | R.P.] |
|------------|-----|------|-------|

Pectate of Lead was prepared by adding a solution of acetate of lead to one of pectate of ammonia, which was slightly alkaline to test paper.

This precipitate was composed of—

| | |
|-----------------------|-------------|
| Pectic acid | 51·256 |
| Oxide of lead | 48·744—100· |

[This would give very nearly 168 pectic acid to 160 oxide of lead. It appears, therefore, that the excess of ammonia precipitated uncombined oxide of lead; for a neutral pectate would consist of 168 and 112 oxide; if it had consisted of equal weights of acid

and base, it would have been a subesqui-pectate; the author, nevertheless, states that this salt gives the same composition for pectic acid as the pectates of silver.—R.P.]

Pectate of Copper was obtained by adding a solution of sulphate of copper to one of pectate of ammonia; the compound is a bright green jelly, which, after drying till it loses no more weight, forms a yellow mass. This salt was not of a constant composition. It was also attempted to be procured by decomposing a very ammoniacal pectate by sulphate of copper. The compound remained long dissolved; at last it formed in intense blue jelly, which could not be obtained free from ammonia, even by long washing; it was evidently a double salt.—*Journal de Pharmacie*, xxiv. 205.

PORTRAIT OF PROF. FARADAY.

Our readers will be gratified by learning that a half-length engraving of Professor Faraday will be immediately published by Mr. C. Turner, A.R.A., Warren-street, Fitzroy-square. The likeness is most successful, and the expression admirable; it gives us not only his character, but figure, and is taken as he is usually seen when delivering his lectures.

PORTRAIT OF ROBERT BROWN, F.R.S., &c.

We gladly also call attention to the portrait of our distinguished countryman Robert Brown, engraved with great success by C. Fox, from the excellent picture by Pickersgill, in the possession of the Linnean Society.

Copies of the Engraving may be obtained at the Society's House.

METEOROLOGICAL OBSERVATIONS FOR SEPTEMBER 1838.

Chiswick.—Sept. 1—3. Very fine. 4. Foggy: very fine. 5. Fine: rain. 6. Heavy rain. 7. Showery. 8. Cloudy and cold. 9—12. Very fine. 13, 14. Overcast and fine. 15. Hazy: very fine. 16, 17. Foggy in the mornings: very fine. 18. Fine: overcast. 19. Rain: very fine: drizzly. 20. Cloudy. 21, 22. Foggy: very fine. 23. Slight rain: fine: rain at night. 24. Foggy: heavy rain. 25. Hazy. 26. Foggy: fine. 27. Rain. 28. Foggy: fine. 29. Foggy: rain. 30. Very fine.

On the evening of the 16th, about 8 P.M., a luminous arch was observed ascending from the west and proceeding in an easterly direction; terminating in the zenith, but with an attenuated broad train, bending towards the south.

Boston.—Sept. 1, 2. Fine. 3. Cloudy. 4, 5. Fine. 6. Rain. 7. Fine: rain A.M. 8. Cloudy. 9—12. Fine. 13—15. Cloudy. 16. Fine: lively appearance of northern lights 8 P.M. 17, 18. Cloudy. 19. Fine: rain early A.M. 20, 21. Fine. 22. Foggy. 23. Cloudy: rain at night. 24—26. Cloudy: rain P.M. 27. Rain. 28. Fine. 29, 30. Foggy.

Applegarth Manse, Dumfries-shire.—Sept. 1. Temperate. 2. Shower A.M.: fair P.M. 3. Fair A.M.: wet P.M. 4. Showery great part of the day. 5. Wet all day. 6. Dripping day. 7—9. Fine harvest days. 10. Cloudy. 11. Cloudy: moist P.M. 12. Showery throughout. 13. Dull: moist. 14. Moist and warm. 15. Mild: warm. 16. Fine harvest day: Aurora. 17. Fine harvest day. 18. Fair till 6 P.M. rain. 19. Fair A.M.: rain. 20. Fair till 3 P.M.: rain and thunder. 21. Fair till 5 P.M.: rain. 22. Showery all day. 23. Fair all day. 24. Dripping day: cold. 25. Very moist. 26. Remarkably fine. 27. Very mild: warm P.M. 28. Fine harvest day. 29. Rather moist. 30. Fine harvest day.

| Days of Month.
1838.
Sept. | Barometer. | | | | Thermometer. | | | | Wind. | | | | Rain. | | | | Dew-point.
Land.
Roy. Soc.
9 a.m. |
|----------------------------------|--------------------------------|-----------|--------|-----------------------|-----------------|-------|--------------------------------------|-----------------------------|-----------|-------|----------------------|-----------------|--------------------------------|-----------|------------|-----------------|--|
| | London:
Roy. Soc.
9 a.m. | Chiswick. | | Boston.
8 1/2 a.m. | Dumfries-shire. | | London:
Roy. Soc.
Fähr. 9 a.m. | Self-register.
Max. Min. | Chiswick. | | Boston
8 1/2 a.m. | Dumfries-shire. | London:
Roy. Soc.
9 a.m. | Chiswick. | Boston. | Dumfries-shire. | |
| | | Max. | Min. | | Max. | Min. | | | | | | | | | | | |
| 1. | 30.108 | 30.111 | 30.076 | 29.49 | 29.92 | 29.97 | 61.0 | 69.5 | 54.0 | 73 | 54 | 60.5 | 56 | 52 | W. | NW. | 56 |
| 2. | 30.140 | 30.201 | 30.123 | 29.52 | 29.97 | 30.04 | 61.5 | 68.5 | 57.0 | 71 | 45 | 60 | 56 | 48 | NW. | NW. | 55 |
| 3. | 30.190 | 30.189 | 30.090 | 29.58 | 29.99 | 29.98 | 60.0 | 68.0 | 53.0 | 73 | 38 | 59 | 50 | 52 | W. | W. | 53 |
| 4. | 29.998 | 29.994 | 29.636 | 29.43 | 29.86 | 29.75 | 59.0 | 69.0 | 52.0 | 76 | 43 | 60 | 56 | 52 | calm | calm | 55 |
| 5. | 29.612 | 29.605 | 29.374 | 29.01 | 29.51 | 29.37 | 64.0 | 69.0 | 59.8 | 75 | 56 | 63.5 | 56 | 52 | S. | SE. | 61 |
| 6. | 29.290 | 29.321 | 29.272 | 28.68 | 29.26 | 29.20 | 59.2 | 69.0 | 58.0 | 75 | 54 | 59 | 56 | 51 | SW. | W. | 59 |
| 7. | 29.344 | 29.473 | 29.318 | 28.80 | 29.33 | 29.57 | 63.5 | 64.5 | 57.4 | 70 | 50 | 60 | 49 | 47 | SW. | N. | 60 |
| 8. | 29.640 | 30.058 | 29.627 | 29.14 | 29.76 | 29.93 | 52.0 | 68.0 | 52.0 | 56 | 35 | 52.5 | 49 | 44 | NNE. | N. | 51 |
| 9. | 30.220 | 30.370 | 30.207 | 29.64 | 30.11 | 30.32 | 51.5 | 56.0 | 44.5 | 63 | 36 | 48 | 47 | 42 | SW. | NW. | 46 |
| 10. | 30.406 | 30.549 | 30.401 | 29.93 | 30.33 | 30.42 | 51.5 | 59.5 | 46.5 | 68 | 34 | 48 | 46 | 46 | NW. | calm | 49 |
| 11. | 30.548 | 30.547 | 30.543 | 30.02 | 30.42 | 30.35 | 48.5 | 59.5 | 45.0 | 70 | 37 | 52 | 46 | 52 | NW. | calm | 47 |
| 12. | 30.500 | 30.515 | 30.418 | 29.93 | 30.30 | 30.26 | 53.2 | 60.0 | 47.5 | 72 | 44 | 57 | 56 | 56 | SW. | calm | 50 |
| 13. | 30.328 | 30.341 | 30.235 | 29.74 | 30.15 | 30.00 | 57.8 | 65.0 | 51.5 | 66 | 43 | 59 | 56 | 56 | W. | calm | 53 |
| 14. | 30.146 | 30.149 | 30.069 | 29.57 | 29.99 | 29.98 | 61.0 | 64.0 | 52.5 | 69 | 56 | 58 | 58 | 52 | SW. | calm | 55 |
| 15. | 30.088 | 30.082 | 30.057 | 29.53 | 30.00 | 30.02 | 59.5 | 65.0 | 59.0 | 70 | 39 | 63 | 54 | 58 | NW. | calm | 59 |
| 16. | 30.078 | 30.105 | 30.037 | 29.55 | 30.03 | 30.05 | 55.8 | 65.2 | 50.8 | 74 | 48 | 57 | 60 | 54 | N. | calm | 55 |
| 17. | 30.072 | 30.121 | 30.037 | 29.56 | 30.11 | 30.11 | 59.3 | 67.7 | 55.3 | 70 | 50 | 58 | 57 | 47 | NNE. | E. | 55 |
| 18. | 30.056 | 30.040 | 30.017 | 29.59 | 30.10 | 30.00 | 57.3 | 67.8 | 52.4 | 64 | 52 | 56.5 | 50 | 46 | NNE. | E. | 55 |
| 19. | 29.948 | 29.942 | 29.845 | 29.59 | 29.90 | 29.70 | 54.4 | 60.4 | 54.6 | 63 | 52 | 57 | 52 | 52 | NW. | W. | 54 |
| 20. | 29.858 | 29.941 | 29.845 | 29.34 | 29.68 | 29.70 | 57.6 | 58.6 | 54.6 | 68 | 35 | 51 | 50 | 48 | SW. | calm | 54 |
| 21. | 29.850 | 29.977 | 29.893 | 29.35 | 29.80 | 29.85 | 50.7 | 63.8 | 46.3 | 68 | 36 | 51 | 48 | 46 | S. | calm | 49 |
| 22. | 30.000 | 30.005 | 29.893 | 29.50 | 29.84 | 29.70 | 53.7 | 60.2 | 42.8 | 69 | 49 | 48 | 50 | 52 | SSW. | calm | 50 |
| 23. | 29.918 | 29.928 | 29.866 | 29.29 | 29.63 | 29.70 | 57.6 | 60.6 | 52.7 | 69 | 45 | 59 | 54 | 52 | SW. | W. | 54 |
| 24. | 29.918 | 29.920 | 29.750 | 29.44 | 29.81 | 29.83 | 54.7 | 63.7 | 51.0 | 56 | 49 | 52 | 56 | 49 | N. | calm | 53 |
| 25. | 29.820 | 29.940 | 29.734 | 29.34 | 29.87 | 29.89 | 51.6 | 56.3 | 50.4 | 58 | 42 | 55 | 52 | 50 | W. | calm | 52 |
| 26. | 29.988 | 29.982 | 29.975 | 29.53 | 29.87 | 29.92 | 48.8 | 55.3 | 47.3 | 58 | 48 | 51 | 52 | 50 | SSW. | NNE. | 52 |
| 27. | 29.864 | 29.933 | 29.841 | 29.46 | 29.92 | 29.85 | 52.6 | 58.3 | 49.2 | 59 | 40 | 53.5 | 54 | 50 | NW. | calm | 53 |
| 28. | 30.000 | 30.072 | 29.975 | 29.48 | 29.93 | 30.00 | 46.3 | 54.7 | 45.0 | 65 | 51 | 49 | 52 | 54 | S. | calm | 47 |
| 29. | 30.016 | 30.138 | 30.014 | 29.58 | 30.00 | 30.02 | 56.3 | 61.0 | 46.5 | 60 | 54 | 51 | 54 | 50 | N. | calm | 53 |
| 30. | 30.200 | 30.303 | 30.197 | 29.68 | 30.16 | 30.29 | 57.8 | 59.2 | 56.4 | 64 | 43 | 51 | 52 | 52 | NW. | calm | 53 |
| Mean. | 30.005 | 30.061 | 29.944 | 29.46 | 29.92 | 29.92 | 55.9 | 62.6 | 51.5 | 67.06 | 45.26 | 55.3 | 53 | 50 | Sum. 2.247 | 1.82 | Mean. 53.2 |

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AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

D E C E M B E R 1838.

LIII. *On the Galvanic Spark.* By Prof. Dr. JACOBI. (*Read before the Imperial Academy of Sciences of St. Petersburg, on the 27th of April 1838*)*.

MR. FARADAY (Experimental Researches, art. 915.†) regards as an essential support of the chemical theory of the galvanic circuit, the galvanic spark, which is stated to be obtained by a process, which he subsequently describes in detail (art. 956.), at the closing of a single galvanic circuit “before contact of the different metals is made; in fact at that moment when chemical forces only are efficient as a cause of action.” In a subsequent series of his experimental researches (1074.) Mr. Faraday does not, it is true, formally retract this position, but he takes the opportunity of explaining the phenomena, which under certain circumstances are exhibited on the closing of a galvanic circuit, in a different manner, equally natural and ingenious. He observes, namely, “The moment they (two metallic surfaces) come in contact, the current passes, it heats, ignites, and even burns the touching points; and the appearance is as if the spark passed on making contact, whereas it is only a case of ignition by the current, contact being previously made, and is perfectly analogous to the ignition of a fine platina wire connecting the extremities of a voltaic battery ‡.” Immediately after the discovery of the so-called *closing spark*, I repeated these experi-

* From the *Bulletin Scientifique de l'Acad. Imp. St. Petersbourg*, T. iv. No. 7. From a copy kindly communicated by the Author. Translated by Mr. W. FRANCIS.

† Prof. Faraday's Eighth Series of Researches here referred to, will be found in Lond. and Edinb. Phil. Mag., vol. vi.

‡ This extract is from Prof. Faraday's Ninth Series, of which an abstract will be found in L. & E. Phil. Mag., vol. vi. p. 301.

ments at Königsberg, and found them confirmed in a very striking way if quicksilver was employed in completing the circuit. However the closing spark appeared to me even at that time doubtful, and in contradiction to many other experiments; first because the free electrical tension at the poles of the voltaic battery, and especially of a single circuit, is too weak to be able to break through a stratum of air; and then, when the spark has once passed, no reason exists why this should not take place in continuance; in other words, a continual spark must originate when the distance of the points remains the same. By the subsequent explanation of Faraday, this subject is perfectly settled; however, in the *Repertorium der Physik*, edited by Professors Dove and Moser, both in the first and second part (I. p. 190. II. p. 113.) the closing spark is still represented as something actual, and discussed as such. In the same work, p. 190, several other experiments are enumerated, in order to show, in the contractions of the frog, phenomena analogous to the closing spark. These experiments, however, are too much confused to be of any importance with respect to the subject in question or any other. It is further stated as a remarkable difference, that the smaller the connecting wire between the exciting plates, the stronger is the spark at closing, and the more insignificant at breaking contact. This singularity is also reduced to a normal phenomenon by Faraday's explanation, as the phenomena of combustion are always more intense when there is less opposition to conduction; therefore a shorter wire in the circuit has the effect in question. Finally, one can scarcely consider the arrangement given by Linari for the production of a thermo-magnetic spark as an apparatus which, as is the case in the above-mentioned work, ii. p. 113, can be employed to decide this point, so important for the theory, but yet so intricate, whether in reality a spark can take place *previously to contact*.

It appeared to me more fit, since in the end all depends on a positive determination, to fix to a beam-compass (*Stangen-zirkel*) with a micrometer screw two very finely pointed wires, which in this way might be very accurately and gradually approximated to each other. Their distance and the other phenomena were measured and observed with a microscope with micrometrical apparatus. For the latter I am obliged to the kindness of M. Lenz, who was also so good as to witness the experiment. I employed a battery of zinc and platina consisting of 12 plates, each of 24 English square inches surface, which might also be connected into a single pair of plates by means of a 2' square surface of platina. The

zinc plates were carefully amalgamated, and the charge was composed of 100 measures of water and 8 of sulphuric acid.

It resulted from frequently repeated experiments that the points might be approximated to within a distance of 0.00605 English inch of each other without a spark passing: the battery might be either connected into a single pair of plates, or arranged as a pile of twelve pairs. Instead of the copper points a small copper ball of 0.1 inch in diameter was now screwed to one side, and even then no spark passed at the above-mentioned distance; the same was the case after the ball had been amalgamated so strongly that a layer of bright fluid mercury still adhered to it. It must therefore be admitted that with a distance of the surfaces of contact of 0.00005" English, no evident spark yet passes, either on employing a simple circuit, or with a battery of 12 pairs. I also inserted in the galvanic circle a very sensitive galvanometer (Nobili's) with double needle, at the distance of the surfaces of contact above stated, when the needle was not in the least affected. Since however the prepared frog passes for a still more sensitive galvanometer, it would be desirable to ascertain at what distance of the surfaces of contact this would exhibit contractions. Not less desirable is it to know the distance at which Professor Moser has seen the thermomagnetic spark and that of the Becquerel's circuit pass (ii. p. 113.).

If therefore, provisionally, a spark previous to contact is not proved, the reason already stated falls to the ground, and no other exists for concluding on a particular direction of the particles of oxygen towards the zinc which takes place even previous to complete contact (i. p. 190.).

What takes place when the contact is completed and broken again, is therefore simply a phenomenon of combustion. We may, however, plainly observe how the surfaces of contact immediately become coloured, or under circumstances red hot. It depends therefore on the proportion of the heating power of the current to the magnitude of the surfaces of contact, whether immediately a larger or smaller particle is ignited and detached. It is always observed that when immediately at contact a spark has been produced, the surfaces of contact have again, after the combustion, separated from each other. The colour of the spark on employing copper points was at first green; if the experiment was frequently repeated, it appeared to me afterwards darkish red; after more frequent repetition, the layer of oxide formed prevented the metallic contact and no further spark appeared. This layer of oxide is pro-

bably the cause also that at the opening of the circuit frequently no spark is produced, when on making contact it had taken place. On making contact, the external metallic surfaces melt or soften together to a certain extent. By an adequate force of the current it requires more revolutions of the micrometer screw to separate them than was necessary to bring them into contact; we can also perceive under the microscope particles which, as it were, are drawn out in the form of a wire. As soon as the dimensions of the particles answer to the heating power of the pile, they burn with the usual phænomena of combustion. We can therefore easily imagine conditions in which all these phænomena are absent; for instance, when the current is weak, or the surfaces of contact large and powerful conductors of heat. Sometimes we also perceive two *separation sparks*, which probably are produced by two places of contact separated one after the other.

The phenomenon may now also be explained, that the separation spark appears more intense by the use of an electro-magnet, or of an electro-magnetic spiral. For here the heating effects of the electro-magnetic and magneto-electric currents combine. The disappearance of the magnetism by which, as is well known, a magneto-electric current is produced, takes place, disregarding other circumstances inherent in the qualities of the iron, for this reason, not instantaneously, because contact can never be suddenly broken. For, the more the surfaces of contact are pressed closely together, forming what is usually termed perfect contact, the more do they also increase in extension, or the smaller is the opposition to conduction. If we break the contact, this, properly speaking, is merely a gradual diminution of the surfaces of contact. By this consequently the opposition to conduction is increased; with this decreases contemporaneously the force of the galvanic current and of the electro-magnetism. The magneto-electric current takes place in the surrounding spiral, which however increases in intensity inversely to the decrease of the original magnetism, and is exalted until its heating power corresponds to the magnitude and other qualities of the exterior metallic surfaces of contact. Then originates the process of combustion, which, however, now also causes the complete separation of the surfaces of contact, and thus entirely interrupts the magneto-electrical circle. The combustion owing to the galvanic current takes place, we have seen, on perfecting the contact, only when the points are fine, or the surfaces of contact small; in this case however the current also becomes feeble, and the magnetism developed in

the spiral is weak only, and the part even which the magneto-electric current plays in the process of combustion is also slight. This part is, with regard to the opposed directions, at the contact, of the galvanic and of the magneto-electric current, even possibly negative. However, it may be difficult to demonstrate by direct observation the modification which connects the process of combustion, at the contact, when an electro-magnet is situated in the circle of the circuit, presupposing, that is, that the power of the galvanic current is in both cases the same, which equality must be produced by an increase in some of the electromotors corresponding to the induced opposition of conduction of the spirals.

LIV. *On some apparent Exceptions to the Law, that like Crystalline Forms indicate like Chemical Formulæ.* By JAMES F. W. JOHNSTON, M.A., F.R.SS. L. & E., F.G.S., &c. &c. *Prof. Chem. and Min., University, Durham.**

FOLLOWING up the subject of a former paper on a supposed analogy in atomic constitution between the earthy carbonates and the alkaline nitrates †, I proceed to examine certain other cases of isomorphous coincidence between substances of which the chemical formulæ according to received views are wholly irreconcilable.

The first exception to the law that substances which replace each other may be represented by analogous formulæ was deduced by Mitscherlich from the observation that ammonia with an atom of water ($\text{NH}_3 + \text{HO}$) may replace potash (KO) in its salts, without changing their crystalline form. Six years have since elapsed and we are still without *conclusive proof* of any other mutual replacement of substances represented by unlike formulæ. There are indeed one or two cases in which appearances seem to render such replacement *probable*, but as yet no other case has been established.

We are acquainted however with a considerable number of substances, which though unlike in constitution, and incapable, or not known to be capable, of replacing each other, yet crystallize in forms which are almost or altogether identical. In the following table, extracted from my Report on Dimorphous bodies ‡, I have arranged all the cases which have hitherto come to my knowledge.

* Communicated by the Author. † See this Journal, xii. p. 480.

‡ Report of the British Association, vol. vi. p. 175. In page 209 of the Report on Dimorphous bodies, line 5, for *Isomorphism* read *Isomerism*.

| Groups. | Formulae. | Common Form. | Authority. |
|---|--|---|--|
| 1. Ammonia, with 1 equiv. of water. }
Potash..... | $\text{NH}_3 + \text{H}_2\text{O}$
K O | ?
? | Mitscherlich. |
| 2. Disulphuret of Copper }
Sulphuret of Silver | Cu_2S
Ag S | { Octohedrons and Rhom. of $71^\circ 30'$ }
Octohedrons. | H. and G. Rose.
They replace each other in Fahlerz. |
| 3. Native Sulphur ... }
Bisulphate of Potash from solution | S
$\text{K S} + \text{H S}$ | Ob. Rh. Pr.
do. | Mitscherlich. |
| 4. Fused Bisulph. Pot. }
Felspar | $\text{K S} + \text{H S}^?$
$\text{K S} + \text{Al Si}^3$ | Ob. Rh. Pr.
do. | |
| 5. Calc Spar }
Nitrate of Potash | $\text{Ca O} + \text{C O}_2$
$\text{K O} + \text{N O}_3$ | Rhomboid.
do. | Frankenheim (Pog. <i>Annal.</i> , xl. 447.) |
| Nitrate of Soda... }
6. Arragonite }
Nitrate of Potash | $\text{Na O} + \text{N O}_3$
$\text{Ca O} + \text{C O}_2$
$\text{K O} + \text{N O}_3$ | do.
Rt. Rh. Pr.
do. | Mitscherlich. |
| 7. Sulphate of Soda }
Permangan. of }
Baryta | $\text{Na O} + \text{S O}_3$
$\text{Ba O} + \text{Mn}_2\text{O}_7$ | Rt. Rh. Pr.
do. | |
| 8. Euclase | $\text{Be Si}^2 + 2 \text{Al Si}$ | Ob. Rh. Pr. | { Brooke, L., and Ed. Phil. Mag., x. 266 and xii. p. 406.† |
| Zoisite..... | $\text{Ca}^3 \text{Si} + 2 \text{Al Si}$ | do. | |
| 9. Silica | $\text{Si O}_3^?$ | Rhomb of $94^\circ 15'$ | |
| Chabasic'..... | $\text{Ca}^3 \left\{ \begin{array}{l} \text{Si}^3 + 3 \text{Al Si}^2 + 10 \text{H} \\ \text{Na}^3 \\ \text{K}^3 \end{array} \right.$ | do. $94^\circ 46'$ | { Johnston, Ibid. ix. p. 166. |
| 10. Mohsite'..... | $\text{Fe, Te, Fe}^?$ | do. $73^\circ 43'$ | |
| Eudyalite | $\text{Na Cl} + (\text{Na}^3 \text{Si}^2 + \text{Ca}^3 \text{Si}^2 + \text{Zr Si} + \text{Fe Si})$ | do. $73^\circ 40'$ | |
| 11. Baryta Harmotome | $2 \text{Ba}^3 \left\{ \begin{array}{l} \text{Si}^4 + 7 \text{Al Si}^3 + 36 \text{H} \\ \text{Ca}^3 \end{array} \right.$ | Rt. Rh. Pr. M M' = $92^\circ 41'$? | Köhler, Pog. <i>Ann.</i> xxxvii. 572. |
| Lime do. | $\text{Ca}^3 \left\{ \begin{array}{l} \text{Si}^2 + 4 \text{Al Si}^2 + 18 \text{H} \\ \text{K}^3 \end{array} \right.$ | do. | do. |
| Stilbite (Desmin.) | $\text{Ba}^3 \left\{ \begin{array}{l} \text{Si} + \text{Al Si}^3 + 6 \text{H} \\ \text{Ca}^3 \end{array} \right.$ | do. $91^\circ 16'?$ † | do. |

* I have marked this formula as doubtful, for though the abstract of Mitscherlich's paper in Poggendorff's *Ann.* xxxix. p. 196, is silent on the subject, yet Berzelius (*Arsberättelse*, 1837, p. 126) says that the fused crystals have a different composition. The Berlin Transactions containing Mitscherlich's entire paper have not yet reached this country.

† The plane M on Euclase assumed by Phillips and Levy as the primary has not been observed on zoisite, but when in like position the corresponding planes on the two minerals have the same angular relations. Thus the two angles observed by Phillips in Euclase to measure $107^\circ 20'$ and $121^\circ 30'$ respectively, were found by Brooke in Zoisite to be $107^\circ 21'$ and $121^\circ \left\{ \begin{array}{l} 30' \\ 45' \end{array} \right.$. Both also possess the brilliant cleavage from which Euclase derives its name.

‡ Levy gives M on M in Harmotome and Stilbite respectively $110^\circ 0'$ and $94^\circ 11'$. The former must obviously refer to other planes than these of Köhler.

Supposing the law to be universally true that like forms indicate like formulæ, let us inquire what changes in the received formulæ for the several members of each of the above groups it would be necessary to introduce, in order to remove the discrepancies which they exhibit.

1. If KO be replaced by $\text{N H}_4 \text{O}$, it will be necessary to suppose potassium a compound metal, and that it is *truly* represented by a combination $\text{R R}'_4$ of two perhaps hitherto unknown elements.

2. The consequence to be deduced from the mutual replacement of the sulphurets of copper and of silver has been stated at length in a former paper*. It appears to imply that both sulphurets must be analogous in constitution, and as leading to fewer changes, that the sulphuret of silver is a disulphuret $\text{Ag}_2 \text{S}$.

This again implies that the received atom of silver must be halved, as was already suggested by its specific heat; and as a consequence, that those of gold, soda, and potash must be halved also. The facts on which this latter consequence rests are stated in the paper above referred to.

3. The next case, that native sulphur has the same form as bisulphate of potash, seems to indicate only that sulphur, so far from being a simple body, is more complicated in its constitution even than potassium, as indicated by the first group. If water may replace potash, then the formula for the bisulphate becomes $\text{R } \ddot{\text{S}}$, which, to bring it into conformity with the supposed law, would require in sulphur a composition represented by $\text{R R}' + \text{R}'' \text{R}'_3$.

4. If from the two formulæ in group 4° we take away the atom of potash common to both, we have $2 \ddot{\text{S}} + \ddot{\text{H}}$, and $4 \ddot{\text{Si}} + \ddot{\text{Al}}$ respectively. By what hypothesis these two formulæ are to be assimilated it is almost needless to inquire. Any method we could adopt would be at least as much opposed to received views as those suggested by the cases already considered.

5. Can the formulæ for nitrate of potash and calc spar be reconciled? In this Journal, vol. xii. p. 480, will be found a short paper in reply to this question. It is there shown that if the atom of potassium be halved, as our second group suggests, and if that of Berzelius for nitrogen be adopted, the formula for nitre would become $\text{K } \ddot{\text{N}}$, or taking the positive elements together $\text{R}_4 \text{O}_6$. That of calc spar is $\text{Ca } \ddot{\text{C}}$, or taking

* Lond. and Edinb. Phil. Mag. vol. xii. p. 324.

the positive elements together $R_2 O_3$, when the ratio is the same as in nitre on the above supposition. This would imply that compounds may be isomorphous, may possibly replace each other, of which the formulæ are only so far analogous that the ratio between the positive and negative elements is the same in both. How far this is true must be determined by experiment; it at least points out a new line along which our inquiries may proceed.

6. In the sixth group the formulæ have precisely the same relation as in the fifth.

7. In the seventh group the formula $\text{Na} \overset{\cdot\cdot\cdot}{\text{S}}$ has to be assimilated to $\text{Ba} \overset{\cdot\cdot\cdot}{\text{Mn}}$. This Dr. Clark proposes to do by *doubling* the atom of sodium, and representing the anhydrous sulphate of soda by $\text{Na} \overset{\cdot\cdot\cdot}{\text{S}}$. The ratio of the positive to the negative elements in this formula is the same (3 : 8) as in the permanganate of baryta.

The analogy thus established between the formulæ is of the same kind as that above arrived at in regard to the members of the fifth group. It is obtained however by an inverse process by *doubling* instead of *halving* the atom of sodium. It is of importance to mark this fact, as it shows that besides the changes in received views which the reconciling of these formulæ would require, they do not all suggest *like* changes, and therefore any they do indicate ought to be looked upon with great suspicion. Dr. Clark has not remained satisfied with the distant analogy obtained by his method of reconciling these two formulæ. He pushes it still further, and to bring, not the whole formulæ merely, but each member of them, into exact correspondence, he represents the composition

of his two salts by the *more rational* (?) formulæ $\text{Na} \overset{\cdot\cdot\cdot}{\text{S}}$ and

$\text{Ba} \overset{\cdot\cdot\cdot}{\text{Mn}}$, supposing that the whole of the oxygen enters into the composition of the acid, and that this acid thus *suroxidized* combines directly with the metal. It is unnecessary thus to strain isolated facts, or to attempt to remodel our entire views in regard to the salts on the faith of one solitary exception. The most important conclusion at which we shall arrive from the study of the relations among the several groups in our table will be, that from none of them can we draw any *positive* knowledge, though all of them afford hints which may lead to new inquiries.

8. The identity of the forms of euclase and zoisite, according to the measurements of Mr. Brooke, has given rise to

some observations by my friend Mr. Richard Phillips*, as if it in some way opposed the general doctrine of isomorphism.

The second member of the formulæ for these two minerals is identical, and may therefore be neglected; the first members are $\underline{\text{Be}} \ddot{\text{Si}}^3$ and $\text{Ca}^3 \ddot{\text{Si}}$ respectively. If from each we take an atom of silica, then $\underline{\text{Be}} \ddot{\text{S}}$ should be capable of replacing Ca^3 ; and if we suppose that glucina is not a *sesqui*, but a *protoxide*, "the supposition," according to Mr. Phillips, "most favourable to the doctrine of isomorphism;" and that the $\underline{\text{Be}}$ of the formula should be written Be^3 , replacing Ca^3 , we should still have an equivalent of silica $\ddot{\text{Si}}$ in excess, in the formula for euclase. It does not appear therefore that the formulæ in the eighth group are at present to be reconciled. Were this the only case of such disagreement in formulæ, we might suspect that Mr. Brooke had mistaken one or other of the minerals, euclase or zoisite; but the number of examples which the table contains tends to remove such suspicion from the mind of those who are not so well acquainted with Mr. Brooke's accuracy and knowledge of minerals as I have had the opportunity of becoming.

9. If the forms of silica and chabasie be identical, their received formulæ appear irreconcilable.

10. Mohsite has not been analysed, but it is not easy to suppose its known constituents to be present in quantities, such as to produce a formula analogous to that of eudyalite.

11. The only relation observable among the formulæ in the 11th group is, that the second and third added together make up the first, or that the baryta harmotome may be supposed to consist of an equivalent of stilbite united to one of lime harmotome. In what way the formulæ for these two minerals are to be reconciled does not appear.

From this discussion then of the mutual relations of the formulæ by which the substances contained in the above groups are severally represented, we have arrived at three conclusions:

1. That some of them appear to be altogether irreconcilable.

2. That those which may be reconciled require the adoption of hypotheses which are much at variance with received opinions; and,

3. That these hypotheses may be directly the converse of each other.

If therefore we can depend on the chemical analyses and the crystalline measurements of the above substances, it is not universally true that like crystalline forms indicate like chemical formulæ.

Does this conclusion invalidate the general doctrine of isomorphism, or present a stumbling-block in the way of received opinions on that subject? By no means. It forms, *if correct*, a distinct and independent as it is an important accession to our knowledge.

To place this statement in its true light, let us consider what has been the general progress of the doctrine of isomorphism, and in what position it really stands.

1. It was observed that the sulphates, the carbonates, &c. of entire classes of bases crystallized in forms which were specific not to each base, as magnesia and baryta, but to each *class* of bases, two of which were represented by magnesia and baryta as their types.

2. A similar observation was made in regard to certain salts containing different acids, as the arsenic and phosphoric; the same form being observed not merely in the arseniate and phosphate of the same base, but in such salts also of each *class* of bases.

3. It was therefore inferred, abstraction being made of what is common to the chemical constitution of such as have like forms, that the remainders have the same form also; that the whole class of bases, for example, lime, magnesia, &c., had the same form, and that the class of acids, the arsenic, phosphoric, and antimonie, had also the same form, or were *isomorphous*.

4. This inference was confirmed by the subsequent observation that the several members of these and other classes of bases and acids might take the place of (replace) each other in quantities which might vary, but which were always proportional to their equivalents, without affecting the form of the resulting crystal.

5. But it was further observed that certain other substances appeared to replace each other, the forms of which had not been determined; that oxide of chromium, for example, might replace alumina; and hence it was inferred that such compounds must also be isomorphous; an inference which in the case of the oxide of chromium was confirmed by the examination of the crystals of this substance subsequently obtained by Wöhler.

6. Now in all these cases, substances having the same form or replacing each other, were found, or inferred to be, analogous in chemical constitution; to be oxides, sulphurets, &c.

which might be represented by the same general formula. It was natural, therefore, to reason in an inverse order, and to ask, if the same formula so generally follows or attends upon the same form, may not all compounds of which the form is alike be represented by like formulæ;—when put in a *rational* form, should not the equivalents deduced from a correct analysis give like formulæ for minerals alike in form? In this idea was presented a new and beautiful test of the accuracy of mineral analyses. Guided by it, Abieh, under the direction of Mitscherlich, examined the family of octohedral minerals, of which magnetic iron is the type, and found that spinella, gahnite, chrome, iron, &c. may all be represented by the same general formula $\bar{R} \bar{R}$. Other cases of a similar kind afterwards occurred, so that it began to appear, and to be thought by many, that like forms *always* indicate like formulæ.

7. But then came forward one after another the anomalies or exceptions to this generalization which are grouped together in the above table, and in which the attendance of like formulæ on like forms cannot be distinguished.

By these exceptions, however, the doctrine of isomorphism is not touched, or the evidence in its favour in any way impaired. They establish, or seem to do so, merely the negative position, that like forms do not *always* in compound substances indicate like formulæ; an important and independent truth, which points to some more general law we have still to seek for, but which is perfectly consistent with the fundamental and leading principles of the isomorphic doctrine.

In the two papers published in former numbers of this Journal*, in reference to the second and fourth of the groups in the table, I was preparing the way for the consideration of the general question which has been the subject of the above remarks. The present paper I have delayed till after the publication of the 6th volume of the Reports of the British Association, that I might not anticipate my own report, of which the above table forms a part. Meanwhile my friend Mr. Richard Phillips, shocked at the changes which one of the cases I had considered seemed to indicate, hastened to publish his "Observations on Isomorphism†," some of the remarks in which, I am satisfied, he will now see to have been unnecessary. In reply to the fourth paragraph of these observations, the only one which calls for remark, I would observe, that judging from my own feelings, I am certain that

* Vol. xii. pp. 324 and 480.

† Ibid., p. 407.

none of its opponents are more anxious for the discovery of truth than those whom Mr. Phillips has honoured with the title of *expounders* of the doctrine of isomorphism; we may have an unfortunate prejudice in favour of collecting and collating *all* known facts, before we venture to pronounce a decided opinion upon a difficult question, but we will not yield to them in love for the truth. We are willing to hope, however, that though we seem at present to be searching for the same truth in different directions, we may ultimately find our paths converge to one point, at which point we shall all, expounders and opponents, meet together, and at length agree*.

Durham, Sept. 12, 1838.

LV. *Experimental Researches in Electricity*.—*Eleventh Series*. By MICHAEL FARADAY, Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburg, Florence, Copenhagen, Berlin, &c. &c.

[Continued from p. 367, and concluded.]

¶ v. On *specific Induction*, or *Specific inductive Capacity*.

1252. **I** NOW proceed to examine the great question of specific inductive capacity, i. e. whether different dielectric bodies actually do possess any influence over the degree of induction which takes place through them. If any such difference should exist, it appeared to me not only of high importance in the further comprehension of the laws and results of induction, but an additional and very powerful argument for the theory I have ventured to put forth, that the whole depends upon a molecular action, in contradistinction to one at sensible distances.

The question may be stated thus: suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A at equal distances and uninsulated; A will then induce equally towards B and C. If in this position of the plates some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same? Will the relation of C and B to A be unaltered, notwithstanding the difference of the dielectrics interposed between them?

1253. As far as I recollect, it is assumed that no change

* If Mr. Phillips will look at the formula for chabasie given in the table, his difficulty about the mutual replacement of potash and soda will or ought to disappear.

will occur under such variation of circumstances, and that the relations of B and C to A depend entirely upon their distance. I only remember one experimental illustration of the question, and that is by Coulomb*, in which he shows that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body as the same wire in air. The experiment offered to me no proof of the truth of the supposition, for it is not the mere films of dielectric substances surrounding the charged body which have to be examined and compared, but the *whole mass* between that body and the surrounding conductors at which the induction terminates. Charge depends upon induction (1171. 1178.); and if induction relate to the particles of the surrounding dielectric, then it relates to *all* the particles of that dielectric inclosed by the surrounding conductors, and not merely to the few situated next to the charged body. Whether the difference I sought for existed or not, I soon found reason to doubt the conclusion that might be drawn from Coulomb's result; and therefore had the apparatus made, which, with its use, has been already described (1187, &c.), and which appears to me well suited for the investigation of the question.

1254. Glass, and many bodies which might at first be considered as very fit to test the principle, proved exceedingly unfit for that purpose. Glass, principally in consequence of the alkali it contains, however well warmed and dried it may be, has a certain degree of conducting power upon its surface, dependent upon the moisture of the atmosphere, which renders it unfit for a test experiment. Resin, wax, naphtha, oil of turpentine, and many other substances were in turn rejected, because of a slight degree of conducting power possessed by them; and ultimately shell-lac and sulphur were chosen, after many experiments, as the dielectrics best fitted for the investigation. No difficulty can arise in perceiving how the possession of a feeble degree of conducting power tends to make a body produce effects, which would seem to indicate that it had a greater capability of allowing induction through it than another body perfect in its insulation. This source of error has been the one I have found most difficult to obviate in the proving experiments.

1255. *Induction through Shell-lac.*—As a preparatory experiment, I first ascertained generally that when a part of the surface of a thick plate of shell-lac was excited or charged, there was no sensible difference in the character of the induction sustained by that charged part, whether exerted through

* *Mémoires de l'Académie*, 1787, pp. 452, 453.

the air in the one direction, or through the shell-lac of the plate in the other; provided the second surface of the plate had not, by contact with conductors, the action of dust, or any other means, become charged (1203.). Its solid condition enabled it to retain the excited particles in a permanent position, but that appeared to be all; for these particles acted just as freely through the shell-lac on one side as through the air on the other. The same general experiment was made by attaching a disc of tin foil to one side of the shell-lac plate, and electrifying it, and the results were the same. Scarcely any other solid substance than shell-lac and sulphur, and no liquid substance that I have tried, will bear this examination. Glass in its ordinary state utterly fails; yet it was essentially necessary to obtain this prior degree of perfection in the dielectric used, before any further progress could be made in the principal investigation.

1256. *Shell-lac and air* were compared in the first place. For this purpose a thick hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus (1187, &c.), so as nearly to fill the lower half of the space *o, o* (fig. 1.) between it and the inner ball; and then charges were divided in the manner already described (1198. 1207.), each apparatus being used in turn to receive the first charge before its division by the other. As the apparatus were known to have equal inductive power when air was in both (1209. 1211.), any differences resulting from the introduction of the shell-lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point sought to be sustained. I have already referred to the precautions necessary in making the experiments (1199, &c.); and with respect to the error which might be introduced by the assumption of the peculiar state, it was guarded against as far as possible in the first place, by operating quickly (1248.), and afterwards by using that dielectric as glass or sulphur, which assumed the peculiar state most slowly, and in the least degree (1239. 1241.).

1257. The shell-lac hemisphere was put into app. i., and app. ii. left filled with air. The results of an experiment in which the charge through air was divided and reduced by the shell-lac app. were as follows:

| App. i. | Lac. | App. ii. | Air. |
|---------|------|----------|-------------|
| | | | Balls 255°. |
| 0° | . | . | . |
| | . | . | 304° |
| | . | . | 297 |

| | |
|-------------|---------------|
| App i. Lac. | App. ii. Air. |
|-------------|---------------|

Charge divided.

| | |
|--------------|---------------------------|
| 113° | |
| | 121° |
| 0 | after being discharged. |
| | 7 after being discharged. |

1258. Here 297° , minus 7° , or 290° , may be taken as the divisible charge of app. ii. (the 7° being fixed stem action (1203. 1232.)), of which 145° is the half. The lac app. i. gave 113° as the power or tension it had acquired after division; and the air app. ii. gave 121° , minus 7° , or 114° , as the force it possessed from what it retained of the divisible charge of 290° . These two numbers should evidently be alike, and they are very nearly so, far indeed within the errors of experiment and observation. But these numbers differ very much from 145° , or the force which the half charge would have had if app. i. had contained air instead of shell-lac; and it appears that whilst in the division the induction through the air has lost 176° of force, that through the lac has only gained 113° .

1259. If this difference be assumed as depending entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then this capacity for electric induction would be inversely as the respective loss and gain indicated above; and assuming the capacity of the air apparatus as 1, that of the shell-lac apparatus would be $\frac{176}{113}$, or 1.55.

1260. This extraordinary difference was so unexpected in its amount, as to excite the greatest suspicion of the general accuracy of the experiment, though the perfect discharge of app. i. after the division showed that the 113° had been taken and given up readily. It was evident that, if it really existed, it ought to produce corresponding effects in the reverse order; and that when induction through shell-lac was converted into induction through air, the force or tension of the whole ought to be *increased*. The app. i. was therefore charged in the first place, and its force divided with app. ii. The following were the results:

| | |
|--------------|---------------|
| App. i. Lac. | App. ii. Air. |
|--------------|---------------|

| | |
|--------------|----|
| | 0° |
| 215° | |
| 204 | |

| App. i. | Lac. | App. ii. | Air. |
|-----------------|---------|----------|---------------------------|
| Charge divided. | | | |
| | | 118° | |
| 118° | | | |
| | | | 0 after being discharged. |
| 0 | | | after being discharged. |

1261. Here 204° must be the utmost of the divisible charge. The app. i. and ii. present 118° as their respective forces; both now much *above* the half of the first force, or 102° , whereas in the former case they were below it. The lac app. i. has lost only 86° , yet it has given to the air app. ii. 118° , so that the lac still appears much to surpass the air, the capacity of the lac app. i. to the air app. ii. being as 1.37 to 1.

1262. The difference of 1.55 and 1.37 as the expression of the capacity for the induction of shell-lac seems considerable, but is in reality very admissible under the circumstances, for both are in error in *contrary directions*. Thus in the last experiment the charge fell from 215° to 204° by the joint effects of dissipation and absorption (1192. 1250.), during the time which elapsed in the electrometer operations, between the applications of the carrier ball required to give those two results. Nearly an equal time must have elapsed between the application of the carrier which gave the 204° result, and the division of the charge between the two apparatus; and as the fall in force progressively decreases in amount (1192.), if in this case it be taken at 6° only, it will reduce the whole transferable charge at the time of division to 198° instead of 204° ; this diminishes the loss of the shell-lac charge to 80° instead of 86° ; and then the expression of specific capacity for it is increased, and, instead of 1.37, is 1.47 times that of air.

1263. Applying the same correction to the former experiment in which air was *first* charged, the result is of the *contrary* kind. No shell-lac hemisphere was then in the apparatus, and therefore the loss would principally be from dissipation, and not from absorption; hence it would be nearer to the degree of loss shown by the numbers 304° and 297° , and being assumed as 6° would reduce the divisible charge to 284° . In that case the air would have lost 170° , and communicated only 113° to the shell-lac; and the relative specific capacity of the latter would appear to be 1.50, which is very little indeed removed from 1.47, the expression given by the second experiment when corrected in the same way.

1264. The shell-lac was then removed from app. i. and put into app. ii. and the experiments of division again made. I give the results, because I think the importance of the point justifies and even requires them.

| App. i. | Air. | App. ii. | Lac. |
|-----------------|---------|----------|------------------|
| Balls 200°. | | | |
| 0 | | 0° | |
| 286 | | | |
| 283 | | | |
| Charge divided. | | | |
| | | 110 | |
| 109 | | | |
| | | 0.25 | after discharge. |
| Trace | | | after discharge. |

Here app. i. retained 109°, having lost 174° in communicating 110° to app. ii.; and the capacity of the air app. is to the lac app., therefore, as 1 to 1.58. If the divided charge be corrected for an assumed loss of only 3°, being the amount of previous loss in the same time, it will make the capacity of the shell-lac app. 1.55 only.

1265. Then app. ii. was charged, and the charge divided thus:

| App. i. | Air. | App. ii. | Lac. |
|-----------------|---------|----------|---------------------------|
| 0° | | | |
| | | 256° | |
| | | 251 | |
| Charge divided. | | | |
| 146 | | | |
| | | 149 | |
| a little | | | after discharge. |
| | | | a little after discharge. |

Here app. i. acquired a charge of 146°, while app. ii. lost only 102° in communicating that amount of force; the capacities being, therefore, to each other as 1 to 1.43. If the whole transferable charge be corrected for a loss of 4° previous to division, it gives the expression of 1.49 for the capacity of the shell-lac apparatus.

1266. These four expressions of 1.47, 1.50, 1.55, and 1.49 for the power of the shell-lac apparatus, through the different variations of the experiment, are very near to each other; the average is close upon 1.5, which may hereafter be used as the expression of the result. It is a very important result; and, showing for this particular piece of shell-lac a decided superiority over air in allowing or causing the act of induction, it

proved the growing necessity of a more close and rigid examination of the whole question.

1267. The shell-lac was of the best quality, and had been carefully selected and cleaned; but as the action of any conducting particles in it would tend, virtually, to diminish the quantity or thickness of the dielectric used, and produce effects as if the two inducing surfaces of the conductors in that apparatus were nearer together than in the one with air only, I prepared another shell-lac hemisphere, of which the material had been dissolved in strong spirit of wine, the solution filtered, and then carefully evaporated. This is not an easy operation, for it is difficult to drive off the last portions of alcohol without injuring the lac by the heat applied; and unless they be dissipated, the substance left conducts too well to be used in these experiments. I prepared two hemispheres this way, one of them unexceptionable; and with it I repeated the former experiments with all precautions. The results were exactly of the same kind; the following expressions for the capacity of the shell-lac apparatus, whether it were app. i. or ii., being given directly by the experiments 1.46, 1.50, 1.52, 1.51; the average of these and several others being very nearly 1.5.

1268. As a final check upon the general conclusion, I then actually brought the surfaces of the air apparatus, corresponding to the place of the shell-lac in its apparatus, nearer together, by putting a metallic lining into the lower hemisphere of the one not containing the lac (1213.). The distance of the metal surface from the carrier ball was in this way diminished from 0.62 of an inch to 0.435 of an inch, whilst the interval occupied by the lac in the other apparatus remained 0.62 of an inch as before. Notwithstanding this change, the lac apparatus showed its former superiority; and whether it or the air apparatus was charged first, the capacity of the lac apparatus to the air apparatus was by the experimental results as 1.45 to 1.

1269. From all the experiments I have made, and their constant results, I cannot resist the conclusion that shell-lac does exhibit a case of *specific inductive capacity*. I have tried to check the trials in every way, and if not remove, at least estimate, every source of error. That the final result is not due to common conduction is shown by the capability of the apparatus to retain the communicated charge; that it is not due to the conductive power of inclosed small particles, by which they could acquire a polarized condition as conductors, is shown by the effects of the shell-lac purified by alcohol; and, that it is not due to any influence of the charged state,

formerly described (1250.), first absorbing and then evolving electricity, is indicated by the *instantaneous* assumption and discharge of those portions of the power which are concerned in the phenomena, that effect occurring in these cases, as in all others of ordinary induction by charged conductors. The latter argument is the more striking in the case where the air apparatus is employed to divide the charge with the lac apparatus, for it obtains its portion of electricity in an *instant*, and yet is charged far above the *mean*.

1270. Admitting for the present the general fact sought to be proved; then 1·5, though it expresses the capacity of the apparatus containing the hemisphere of shell-lac, by no means expresses the relation of lac to air. The lac only occupies one half of the space *o, o*, of the apparatus containing it, through which the induction is sustained; the rest is filled with air, as in the other apparatus; and if the effect of the two upper halves of the globes be abstracted, then the comparison of the shell-lac powers in the lower half of the one, with the power of the air in the lower half of the other, will be as 2 : 1; and even this must be less than the truth, for the induction of the upper part of the apparatus, i. e. of the wire and ball B (fig. 1.) to external objects, must be the same in both, and considerably diminish the difference dependent upon, and really producible by, the influence of the shell-lac within.

1271. *Glass*.—I next worked with glass as the dielectric. It involved the possibility of conduction on its surface, but it excluded the idea of conducting particles within its substance (1267.) other than those of its own mass. Besides this it does not assume the charged state (1239.) so readily, or to such an extent as shell-lac.

1272. A thin hemispherical cup of glass being made hot was covered with a coat of shell-lac dissolved in alcohol, and after being dried for many hours in a hot place, was put into the apparatus and experimented with. It exhibited effects so slight, that, though they were in the direction indicating a superiority of glass over air, they were allowed to pass as possible errors of experiment; and the glass was considered as producing no sensible effect.

1273. I then procured a thick flint glass hemispherical cup resembling that of shell-lac (1239.), but not filling up the space *o, o*, so well. Its average thickness was 0·4 of an inch, there being an additional thickness of air, averaging 0·22 of an inch, to make up the whole space of 0·62 of an inch between the inducing metallic surfaces. It was covered with a film of shell-lac as the former was, (1272.) and being made

very warm, was introduced into the apparatus, also warmed, and experiments made with it as in the former instances (1257. &c.). The general results were the same as with shell-lac, i. e. glass surpassed air in its power of favouring induction through it. The two best results as respected the state of the apparatus for retention of charge, &c., gave, when the air apparatus was charged first 1.336, and when the glass apparatus was charged first 1.45, as the specific inductive capacity for glass, both being without correction. The average of nine results, four with the glass apparatus first charged, and five with the air apparatus first charged, gave 1.38 as the power of the glass apparatus; 1.22 and 1.46 being the minimum and maximum numbers with all the errors of experiment upon them. In all the experiments the glass apparatus took up its inductive charge instantly, and lost it as readily; and during the short time of each experiment, acquired the peculiar state in a small degree only, so that the influence of this state, and also of conduction upon the results, must have been small.

1274. Allowing specific inductive capacity to be proved and active in this case, and 1.38 as the expression for the glass apparatus, then the specific inductive capacity of flint glass will be above 1.76, not forgetting that this expression is for a piece of glass of such thickness as to occupy not quite two-thirds of the space through which the induction is sustained (1273. 1253.).

1275. *Sulphur*.—The same hemisphere of this substance was used in app. ii. as was formerly referred to (1242.). The experiments were well made, i. e. the sulphur itself was free from charge both before and after each experiment, and no action from the stem appeared (1203. 1232.), so that no correction was required on that score. The following are the results when the air apparatus was first charged and divided:

| App. i. | Air. | App. ii. | Sulphur. |
|---------|-----------------|----------|--------------------|
| | Balls 280° | | |
| 0° | . . . | | 0° |
| 438 | . . . | | |
| 434 | . . . | | |
| | Charge divided. | | |
| | . . . | 162 | |
| 164 | . . . | | |
| | . . . | 160 | |
| 162 | . . . | | |
| | . . . | | 0 after discharge. |
| 0 | . . . | | after discharge. |

Here app. i. retained 164° , having lost 270° in communicating 162° to app. ii., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1.66.

1276. Then the sulphur apparatus was charged first, thus:

| App. i. | | App. ii. |
|-----------------|---------|--------------------|
| 0° | | 0° |
| | | 395 |
| | | 388 |
| Charge divided. | | |
| 237 | | |
| | | 238 |
| 0 | | after discharge. |
| | | 0 after discharge. |

Here app. ii. retained 238° , and gave up 150° in communicating a charge of 237° to app. i., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1.58. These results are very near to each other, and we may take the mean 1.62 as representing the specific inductive capacity of the sulphur apparatus; in which case the specific inductive capacity of sulphur itself as compared to air = 1 (1270°) will be about or above 2.24.

1277. This result with sulphur I consider as one of the most unexceptionable. The substance when fused was perfectly clear, pellucid, and free from particles of dirt (1267.), so that no interference of small conducting particles confused the result. The body when solid is an excellent insulator, and by experiment was found to take up, with great slowness, that state (1241. 1242.) which alone seemed likely to disturb the conclusion. The experiments themselves, also, were free from any need of correction. Yet notwithstanding these circumstances, so favourable to the exclusion of error, the result is a higher specific inductive capacity for sulphur than for any other body as yet tried; and though this may in part be due to the sulphur being in a better shape, i. e. filling up more completely the space *o, o*, (fig. 1.) than the cups of shell-lac and glass, still I feel satisfied that the experiments altogether fully prove the existence of a difference between dielectrics as to their power of favouring an inductive action through them; which difference may, for the present, be expressed by the term *specific inductive capacity*.

1278. Having thus established the point in the most favourable cases that I could anticipate, I proceeded to examine other bodies amongst solids, liquids, and gases. These results I shall give with all convenient brevity.

1279. *Spermaceti*.—A good hemisphere of spermaceti being

tried as to conducting power whilst its two surfaces were still in contact with the tin-foil moulds used in forming it, was found to conduct sensibly even whilst warm. On removing it from the moulds and using it in one of the apparatus, it gave results indicating a specific inductive capacity between 1.3 and 1.6 for the apparatus containing it. But as the only mode of operation was to charge the air apparatus, and then after a quick contact with the spermaceti apparatus, ascertain what was left in the former (1281.), no great confidence can be placed in the results. They are not in opposition to the general conclusion, but cannot be brought forward as argument in favour of it.

1280. I endeavoured to find some liquids which would insulate well, and could be obtained in sufficient quantity for these experiments. Oil of turpentine, native naphtha rectified, and the condensed oil gas fluid, appeared by common experiments to promise best as to insulation. Being left in contact with fused carbonate of potassa, chloride of lime, and quick lime for some days and then filtered, they were found much injured in insulating power; but after distillation acquired their best state, though even then they proved to be conductors when large metallic contact was made with them.

1281. *Oil of Turpentine rectified.*—I filled the lower half of app. i. with the fluid; and as it would not hold a charge sufficiently to enable me first to measure and then divide it, I charged app. ii. containing air, and dividing its charge with app. i. by a quick contact, measured that remaining in app. ii.: for, theoretically, if a quick contact would divide up to equal tension between the two apparatus, yet without sensible loss from the conducting power of app. i.; and app. ii. were left charged to a degree of tension above half the original charge, it would indicate that oil of turpentine had less specific inductive capacity than air; or, if left charged below that mean state of tension, it would imply that the fluid had the greater inductive capacity. In an experiment of this kind, app. ii. gave as its charge 390° before division with app. i., and 175° afterwards, which is less than the half of 390° . Again, being at 175° before division, it was 79° after, which is also less than half the divided charge. Being at 79° , it was a third time divided, and then fell to 36° , less than the half of 79° . Such are the best results I could obtain; they are not inconsistent with the belief that oil of turpentine has a greater specific capacity than air, but they do not prove the fact, since the disappearance of more than half the charge may be due to the conducting power merely of the fluid.

1282. *Naphtha*.—This liquid gave results similar in their nature and direction to those with oil of turpentine.

1283. A most interesting class of substances, in relation to specific inductive capacity, now came under review, namely, the gases or æriform bodies. These are so peculiarly constituted, and are bound together by so many striking physical and chemical relations, that I expected some remarkable results from them: air in various states was selected for the first experiments.

1284. *Air, rare and dense*.—Some experiments of division (1208.) seemed to show that dense and rare air were alike in the property under examination. A simple and better process was to attach one of the apparatus to an air-pump, to charge it, and then examine the tension of the charge when the air within was more or less rarefied. Under these circumstances it was found, that commencing with a certain charge, that charge did not change in its tension or force as the air was rarefied, until the rarefaction was such that *discharge* across the space *o, o* (fig. 1.) occurred. This discharge was proportionate to the rarefaction; but having taken place, and lowered the tension to a certain degree, that degree was not at all affected by restoring the pressure and density of the air to their first quantities.

Inches of Mercury.

| | | | |
|-----------------------|-----|----------------------|-----|
| Thus at a pressure of | 30 | the charge was | 88° |
| Again | 30 | the charge was | 88 |
| Again | 30 | the charge was | 87 |
| Reduced to | 14 | the charge was | 87 |
| Raised again to..... | 30 | the charge was | 86 |
| Being now reduced to | 3·4 | the charge fell to | 81 |
| Raised again to..... | 30 | the charge was still | 81 |

1285. The charges were low in these experiments, first that they might not pass off at low pressure, and next that little loss by dissipation might occur. I now reduced them still lower, that I might rarefy further, and for this purpose in the following experiment used a measuring interval in the electrometer of only 15° (1185.). The pressure of air within the apparatus being reduced to 1·9 inches of mercury, the charge was found to be 29°; then letting in air till the pressure was 30 inches, the charge still 29°.

1286. These experiments were repeated with pure oxygen with the same consequences.

1287. This result of *no variation* in the electric tension being produced by variation in the density or pressure of the air, agrees perfectly with those obtained by Mr. Harris, and

described in his beautiful and important investigations contained in the *Philosophical Transactions**; namely that induction is the same in rare and dense air, and that the divergence of an electrometer under such variations of the air continues the same, provided no electricity pass away from it. The effect is one entirely independent of that power which dense air has of causing a higher charge to be retained upon the surface of conductors in it than can be retained by the same conductors in rare air; a point I propose considering hereafter.

1288. I then compared *hot and cold air* together, by raising the temperature of one of the inductive apparatus as high as it could be without injury, and then dividing charges between it and the other apparatus containing cold air. The temperatures were about 50° and 200° . Still the power or capacity appeared to be unchanged; and when I endeavoured to vary the experiment, by charging a cold apparatus and then warming it by a spirit-lamp, I could obtain no proof that the inductive capacity underwent any alteration.

1289. I compared *damp and dry air* together, but could find no difference in the results.

1290. *Gases*.—A very long series of experiments was then undertaken for the purpose of comparing *different gases* one with another. They were all found to insulate well, except such as acted on the shell-lac of the supporting stem; these were chlorine, ammonia, and muriatic acid. They were all dried by appropriate means before being introduced into the apparatus. It would have been sufficient to have compared each with air; but, in consequence of the striking result which came out, namely, that *all had the same power of, or capacity for*, sustaining induction through them, (which perhaps might have been expected after it was found that no variation of density or pressure produced any effect,) I was induced to compare them, experimentally, two and two in various ways, that no difference might escape me, and that the sameness of result might stand in full opposition to the contrast of property, composition, and condition which the gases themselves presented.

1291. The experiments were made upon the following pairs of gases.

1. Nitrogen and..... Oxygen.
2. Oxygen Air.
3. Hydrogen Air.
4. Muriatic acid gas..... Air.

* *Philosophical Transactions*, 1834, pp. 223, 224, 237, 244. [See L. and E. Phil. Mag. vol. iv. p. 436.—EDIT.]

| | |
|-------------------------|-------------------------|
| 5. Oxygen | Hydrogen. |
| 6. Oxygen | Carbonic acid. |
| 7. Oxygen | Olefiant gas. |
| 8. Oxygen | Nitrous gas. |
| 9. Oxygen | Sulphurous acid. |
| 10. Oxygen | Ammonia. |
| 11. Hydrogen | Carbonic acid. |
| 12. Hydrogen | Olefiant gas. |
| 13. Hydrogen | Sulphurous acid. |
| 14. Hydrogen | Fluo-silicic acid. |
| 15. Hydrogen | Ammonia. |
| 16. Hydrogen | Arseniuretted hydrogen. |
| 17. Hydrogen | Sulphuretted hydrogen. |
| 18. Nitrogen | Olefiant gas. |
| 19. Nitrogen | Nitrous gas. |
| 20. Nitrogen | Nitrous oxide. |
| 21. Nitrogen | Ammonia. |
| 22. Carbonic oxide..... | Carbonic acid. |
| 23. Carbonic oxide..... | Olefiant gas. |
| 24. Nitrous oxide | Nitrous gas. |
| 25. Ammonia | Sulphurous acid. |

1292. Notwithstanding the striking contrasts of all kinds which these gases present of property, of density, whether simple or compound, anions or cations (665.), of high or low pressure (1284. 1286.), hot or cold (1288.), not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold between bodies having the gaseous condition and form. Another equally important electrical relation, which will be examined in the next paper, is that which the different gases have to each other at the *same pressure* of causing the retention of the *same or different degrees of charge* upon conductors in them. These two results appear to bear importantly upon the subject of electro-chemical excitation and decomposition; for as *all* these phænomena, different as they seem to be, must depend upon the electrical forces of the particles of matter, the very distance at which they seem to stand from each other will do much, if properly considered, to illustrate the principle by which they are held in one common bond, and subject, as they must be, to one common law.

1293. It is just possible that the gases may differ from each other in their specific inductive capacity, and yet by quan-

tities so small as not to be distinguished in the apparatus I have used. It must be remembered, however, that in the gaseous experiments the gases occupy all the space *o, o*, (fig. 1.) between the inner and the outer ball, except the small portion filled by the stem; and the results, therefore, are twice as delicate as those with solid dielectrics.

1294. The insulation was good in all the experiments recorded, except Nos. 10, 15, 21, and 25, being those in which ammonia was compared with other gases. When shell-lac is put into ammoniacal gas its surface gradually acquires conducting power, and in this way the lac part of the stem within was so altered, that the ammonia apparatus could not retain a charge with sufficient steadiness to allow of division. In these experiments, therefore, the other apparatus was charged; its charge measured and divided with the ammonia apparatus by a quick contact, and what remained untaken away by the division again measured (1281.). It was so nearly one half of the original charge, as to authorize, with this reservation, the insertion of ammoniacal gas amongst the other gases, as having equal power with them.

1295. Thus *induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount, but opposite in its direction and tendencies (1164.). Induction requires no sensible thickness in the conductors which may be used to limit its extent; an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states whilst the inductions continue. Nor is it affected by the nature of the limiting conductors, provided time be allowed, in the case of those which conduct slowly, for them to assume their final state (1170.).

1296. But with regard to the *dielectrics* or insulating media, matters are very different (1167.). Their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of *specific inductive capacities*, these differences being in some cases very great.

1297. The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse

force equivalent to a dilatation or repulsion of these representative lines (1224.); or the attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsive or a diverging force in the transverse direction (1304.).

1298. Induction appears to consist in a certain polarized state of the particles, into which they are thrown by the electrified body sustaining the action, the particles assuming positive and negative points or parts, which are symmetrically arranged with respect to each other and the inducing surfaces or particles*. The state must be a forced one, for it is originated and sustained only by force, and sinks to the normal or quiescent state when that force is removed. It can be *continued* only in insulators by the same portion of electricity, because they only can retain this state of the particles (1304.).

1299. The principle of induction is of the utmost generality in electric action. It constitutes charge in every ordinary case, and probably in every case; it appears to be the cause of all excitement, and to precede every current. The degree to which the particles are affected in this their forced state, before discharge of one kind or another supervenes, appears to constitute what we call *intensity*.

1300. When a Leyden jar is *charged*, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. *Discharge* is the return of these particles to their natural state from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.

1301. All charge of conductors is on their surface, because being essentially inductive, it is there only that the medium capable of sustaining the necessary inductive state begins. If the conductors are hollow and contain air or any other dielectric, still no *charge* can appear upon that internal surface, because the dielectric there cannot assume the polarized state throughout, in consequence of the opposing actions in different directions.

1302. The known influence of *form* is perfectly consistent with the corpuscular view of induction set forth. An electrified cylinder is more affected by the influence of the surrounding conductors (which complete the condition of charge) at the ends than at the middle, because the ends are exposed

* The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognised matter. That is a question which I may be induced to consider in the next or following series of these researches.

to a greater sum of inductive forces than the middle; and a point is brought to a higher condition than a ball, because, by relation to the conductors around, more inductive force terminates on its surface than on an equal surface of the ball with which it is compared. Here too, especially, can be perceived the influence of the lateral or transverse force (1297.), which, being a power of the nature of or equivalent to repulsion, causes such a disposition of the lines of inductive force in their course across the dielectric, that they must accumulate upon the point, the end of the cylinder, or any projecting part.

1303. The influence of *distance* is also in harmony with the same view. There is perhaps no distance so great that induction cannot take place through it*: but with the same constraining force (1298.) it takes place the more easily, according as the extent of dielectric through which it is exerted is lessened. And as it is assumed by the theory that the particles of the dielectric, though tending to remain in a normal state, are thrown into a forced condition during the induction; so it would seem to follow that the fewer there are of these intervening particles opposing their tendency to the assumption of the new state, the greater degree of change will they suffer, i. e. the higher will be the condition they assume, and the larger the amount of inductive action exerted through them.

1304. I have used the phrases *lines of inductive force* and *curved lines* of force (1231. 1297. 1298. 1302.) in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is of course the resultant of compound forces, every molecule being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous. The transverse force is merely this relation considered in a direction oblique to the lines of inductive force, and at present I mean no more than that by the phrase. With respect to the term *polarity* also, I mean at present only a disposition of force by which the same molecule acquires opposite powers on different parts. The particular way in which this disposition is made will come into consideration hereafter, and probably varies in different bodies, and so produces variety of electrical relation.

* I have traced it experimentally from a ball placed in the middle of the large cube formerly described (1173.) to the sides of the cube six feet distant, and also from the same ball placed in the middle of our large lecture-room to the walls of the room at twenty-six feet distance, the charge upon the ball in these cases being solely due to induction through these distances.

All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate. Further inquiry, I trust, will enable us by degrees to restrict the sense more and more, and so render the explanation of electrical phænomena day by day more and more definite.

1305. As a test of the probable accuracy of my views, I have throughout this experimental examination compared them with the conclusions drawn by M. Poisson from his beautiful mathematical inquiries *. I am quite unfit to form a judgement of these admirable papers; but as far as I can perceive, the theory I have set forth and the results I have obtained are not in opposition to such of those conclusions as represent the final disposition and state of the forces in the limited number of cases he has considered. His theory assumes a very different mode of action in induction to that which I have ventured to support, and would probably find its mathematical test in the endeavour to apply it to cases of induction in curved lines. To my feeling it is insufficient in its mode of accounting for the retention of electricity upon the surface of conductors by the pressure of the air, an effect which I hope to show is simple and consistent according to the present view; and it does not touch voltaic electricity, or in any way associate it and what is called ordinary electricity under one common principle.

I have also looked with some anxiety to the results which that indefatigable philosopher Harris has obtained in his investigation of the laws of induction †, knowing that they were experimental, and having a full conviction of their exactness; but I am happy in perceiving no collision at present between them and the views I have set forth.

1306. Finally, I beg to say that I put forth my particular view with doubt and fear, lest it should not bear the test of general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with all known facts, and the manner in which it linked together effects apparently very different in kind, urged me to write the present paper. I as yet see no inconsistency between it and nature, but, on the contrary, think I perceive much new light thrown by it on her operations; and my next papers will be devoted to a review of the phænomena of conduction, electrolyzation, current, mag-

* *Mémoires de l'Institut*, 1811, tom. xii. the first page 1, and the second paging 163.

† *Philosophical Transactions*, 1834, p. 213.

netism, retention, discharge, and some other points, with an application of the theory to these effects, and an examination of it by them.

Royal Institution, Nov. 16, 1837.

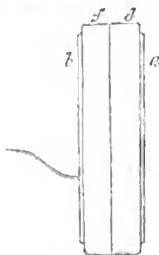


Fig. 11.

Omitted in page 365. (1246.)

LVI. *On a new Voltaic Combination.* By W. R. GROVE, Esq., M.A.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ON first hearing of porous porcelain being employed as a diaphragm for preventing the mutual precipitation of the metals on each other in voltaic combinations, it struck me that one of the plates of metals usually employed might be dispensed with by precipitation upon the other from a metallic solution. After some unsuccessful trials, I constructed a trough as following: a piece of common stout millboard of the length required, and of breadth sufficient to form the bottom and sides, is separated lengthwise into three parallel divisions by cuts one-third through; it is then covered with a thin layer of cement and bent up into the form of a trough; four-inch squares of common sheet iron and unglazed porcelain plates of the same dimensions are then warmed and slid alternately into the trough, as in Cruickshank's form, at about three tenths inch distance*. A solution of sulphate of copper and dilute acid being poured into the alternate cells, a very active series is formed by the precipitation of the copper on one sur-

* Pasteboard is preferable to wood for the formation of these troughs: it has not strength sufficient by its warping to crack the cement, is much more easily constructed, and a better insulator; it should be thinly coated with varnish on the outside to prevent capillary absorption by accidentally touching liquids. The plates of porous ware here mentioned may be conveniently employed for rendering constant the common Cruickshank trough, being warmed sufficiently to melt the cement and slid into the cells.

face of the iron; that which I formed was of twenty plates: the shock, without coils or condenser of any description, was so powerful as to be scarcely tolerable. When a wire was scraped along the edge of the plate at one extremity, the other hand touching the opposite end, the decomposition of water was also rapid, though I have not yet accurately measured its powers: its action continued unabated for nearly three hours without the addition of any acid or sulphate. If greater constancy be required, the alternate cells can be filled up with coarsely powdered sulphate, and some added as required. I was fearful that upon a second trial the intensity would have much abated, in consequence of the oxidated surface of the plates preventing so uniform a deposition of the precipitate, but was gratified to find that, after having been suffered to dry and remain at rest for several days, its action was as intense and constant as upon the first trial. The advantage of this form, where series and sustained power are required, I consider to be its extreme economy, a single cheap metal being employed instead of two expensive ones; the greater durability of iron as compared with zinc; the cutting in squares, so that none is wasted; and the tiresome process of soldering being altogether dispensed with. The diaphragms and solutions are common to every form of constant battery: possibly very thin plates of deal might do as well as porcelain; the durability of the latter material, however, makes its expense unimportant. My object in this communication is not so much to vaunt this particular form of battery as to direct increased attention to the porous filter as likely to form an important element in the analysis of the voltaic trough: it may possibly throw some light upon the organization of the torpedo.

Hoping you will consider this letter of sufficient importance to merit insertion in the *Philosophical Magazine*,

I remain, Gentlemen, yours, &c.

Swansea, Oct. 26, 1838.

W. R. GROVE.

LVII. *On the Reduction of the Chlorides of Mercury when mixed with Organic Substances.* By JOHN B. NEVINS, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

A SHORT time since Mr. West, Lecturer on Chemistry at the Medical School in this town, mentioned to me the fact, that if a mixture of Hg Cl with some vegetable substances be heated, metallic Hg is sublimed; and recommended that the circumstances upon which this depended should be ex-

amined. The results of some experiments on this subject I beg leave to hand you, and to request the favour of their insertion in your Magazine.

When Hg Cl and Hg Cl^2 are mixed with vegetable substances, as bread crumb, sugar, &c., and exposed to heat in a test tube, a series of decompositions more or less complex ensues, and metallic Hg sublimes.

In both these cases the theory of the reduction seems to be the same. Water, or in some cases some other compound of H, is decomposed, and the H, when in the nascent state, combines with the Cl in the compound, forming H Cl , which is driven off, and condensed by the water in the receiver, rendering it distinctly acid to test paper, and which is proved to be H Cl by the usual tests. Water and empyreumatic oil are also formed as in other cases of vegetable decomposition.

The H would seem to be the only essential reducing agent, for when Hg Cl was mixed with pure charcoal, and a few drops of oil of lemons were added, the reduction took place very readily on the application of heat, with formation, as in other instances, of H Cl ; and the same result was obtained when the experiment was repeated with the omission of the charcoal.

The effect of the presence of water in producing the reduction of the Hg was strikingly shown in the following experiments. When dry Hg Cl and charcoal were mixed and exposed to heat, no decomposition ensued; but on the addition of a few drops of water, H Cl and C O^2 were formed and Hg was sublimed.

It seems probable that it is only while in the nascent state that H possesses the power of decomposing Hg Cl , for when a stream of H gas was passed over Hg Cl , heated either by itself or when mixed with charcoal, no reduction took place; while when Hg Cl was introduced into a vessel containing the materials for forming H, it was decomposed similarly to Ag Cl . When Hg Cl^2 was subjected in a similar manner to the action of nascent H, it was first converted into Hg Cl , and afterwards by continuing the process it was reduced to the metallic state.

The same results were obtained in all the experiments, which were made with rhubarb, bread crumb, pure sugar and other vegetable substances, and with moistened charcoal.

In some of these cases, upon boiling the residue after the sublimation of the Hg, a small quantity of Ca Cl was detected; but as even in these cases free H Cl was evolved and collected, it is evident that the Ca was not the reducing agent.

The lowest heat of a spirit-lamp is quite sufficient for effecting the decomposition.

So far as I have been able to observe, there are no differences in the phænomena exhibited in the reduction of Hg from Hg Cl and Hg Cl², or in any of the circumstances connected with the processes, by which we can form any judgement as to which of these compounds we are operating upon.

I am, Gentlemen, yours, &c.

Leeds, June 5, 1838.

JOHN B. NEVINS.

LVIII. *On the Process for obtaining the Bichromate of the Perchloride of Chrome, as viewed under the Microscope.*
By the Rev. EDWARD CRAIG, F.R.S.E.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I OBSERVE in your Number for July, p. 78, a notice of the bichromate of perchloride of chromium, and of the process for obtaining it. It may perhaps be interesting to call attention to the exhibition of this process under the microscope, which certainly throws some light upon the action going on. It presents one of the most beautiful objects ever seen.

Put a very small portion of the muriate of soda on a thin flat glass, and add to it a drop of strong solution of bichromate of potass; lay this on the port-object, and adjust it to the focus of the microscope. Then place upon another flat glass a drop of sulphuric acid, and turn it down upon the other glass. The action commences. The sulphuric acid is seen attacking and breaking down the muriate of soda and setting free the muriatic acid. Crystals of sulphate of soda are speedily formed. The sulphuric acid acts also on the bichromate of potass, and sets free the chromic acid; crystals of sulphate of potass are also formed. Over these crystals of both kinds a multitude of green particles are observed, which are probably a chromate of potass, formed from one equivalent of the chrome in the bichromate, and then the other equivalent of the chrome combines with the disengaged chlorine, and appears in large blood-red globules over the whole field. The operation when steadily watched is very beautiful; and it is one in which the microscope seems to develope with peculiar interest a process whose specific features are lost sight of in the retort. According to the view which is thus given of the process, it seems natural to call the product chlorochromic acid.

I am, Gentlemen, yours, &c.

Burton Latimer, Kettering, Oct. 20, 1838.

EDWARD CRAIG.

Phil. Mag. S. 3. Vol. 13. No. 84. Dec. 1838.

2 F

LIX. *On the Experiments detailed in Mr. Waldie's Paper on Combustion and Flame, inserted in the Lond. and Edin. Phil. Mag. for August 1838.* By D. F. GREGORY, Esq.

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

I ONLY observed yesterday, in your August Number, an article on the subject of combustion, on which, though it be now rather late, I would beg to make some observations. The author of the paper, Mr. Waldie, does not seem to be aware that almost all the experiments which he has there detailed were made several years ago, and are well known to chemists, and frequently exhibited in public lectures. They were originally due to that very ingenious chemist Mr. K. T. Kemp of Edinburgh, Chemical Assistant to Dr. Hope, who first performed them in 1833. I do not know whether or not Mr. Kemp communicated a paper on the subject to any of the Journals, but I know that he communicated them freely to other chemists, who as well as Mr. Kemp have exhibited them frequently in their lecture-rooms since that time, so that they must have become pretty generally known. And I am surprised that Mr. Waldie, who seems to have directed his attention to this branch of chemistry, should not have heard of them before this. I may mention that one of the experiments, that of burning a jet of oxygen in an atmosphere of hydrogen, I saw performed by Dr. Hope in his lecture-room as far back as 1829; but if I recollect rightly, he did not give the true theory of the experiment. Mr. Kemp did this, and illustrated it not only by the experiments detailed by Mr. Waldie, but also by others, which that gentleman does not seem to have tried, such as the burning of chlorate of potass or nitre in an electropositive atmosphere.

If you think these remarks worthy of a place in your pages, their insertion will much oblige,

Gentlemen, yours, &c.

Trinity College, Cambridge,
Nov. 3, 1838.

D. F. GREGORY.

LX. *On a certain Difficulty connected with the Demonstration of Euclid, Book I. Prop. 29.**

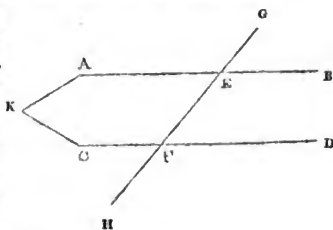
A WELL-known difficulty connected with the demonstration of Euclid, Book I. Prop. 29, which has long been a stumbling-block in the very threshold of mathematical science,

* Communicated by the Author.

may be removed in the following simple manner, by making an alteration in Euclid's definitions. It is a singular fact that the difficulty arose solely from his having adopted an obvious but inadequate definition of parallel lines. The fault of his definition is, that it specifies only a negative property of those lines, viz. that being produced ever so far both ways they do not meet; from which it is plain that nothing positive could be deduced. The following definition may be given in its place.

If a straight line falling on two other straight lines, makes the exterior angle equal to the interior and opposite angle on the same side of it, those lines are said to be parallel.

From the sixteenth proposition it may then be readily deduced that such lines will never meet; for, if possible, let the parallel lines AB , CD meet towards AC in K , then if GEF be the straight line falling upon them, we have by the definition



$$\angle GEA = \angle EFC;$$

but by prop. 16

$$\angle GEA \text{ is greater than the } \angle EFC;$$

which is absurd, therefore the lines do not meet towards AC .

In the same manner it may be proved that they do not meet towards BD ; and therefore they never meet. This proposition will supply the place of the old definition, and the twenty-ninth proposition itself is nearly superseded, for the first part of it is contained in the definition, and the second part follows immediately by combining the definition with the thirteenth proposition.

These few lines may possess some interest for those who are concerned in the controversy about axioms and definitions. It may be proper to state that the idea was suggested to the author by reading Dugald Stewart's remarks on the use and importance of definitions in abstract reasoning. It struck him that the difficulty which Euclid attempts to remove by the introduction of an axiom, should rather, if Dugald Stewart's sentiments were correct, be obviated by some change in the definitions. Whether Euclid's error arose from his ignorance of the philosophy of his subject, or whether he was afraid of offending the prejudices of his tyros by a strange definition of so simple a thing as parallel lines, the writer will

not pretend to decide. Of the justness of Dugald Stewart's views on the subject he is himself fully convinced, but he has not time at present to enter into a question, which indeed appears to have been fully discussed already.

Cambridge, Sept. 18, 1838.

Z. Y.

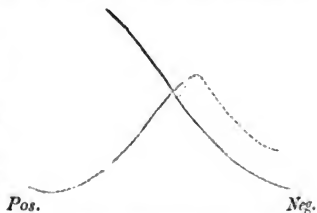
LXI. On a remarkable Difference in the Heat attained by the Electrodes of a powerful Constant Battery. By J. P. GASSIOT, Esq.: in a Letter to Mr. Brayley.

MY DEAR SIR,

THE following result, obtained with an extensive series of the voltaic battery made on the principle of that of Prof. Daniell, may, perhaps, not be uninteresting to you. The battery consists of 160 half-pint earthenware jars, the zinc elements being placed inside, the size permitting the use of brown paper in lieu of membrane, the exciting liquids being saturated solutions of sulphate of copper and common salt.

The effects were of the most brilliant description, and some curious results were obtained, particularly as to its heating power. The following is, I believe, entirely new, and as it in some measure militates against the theory of a double fluid, may, perhaps, be acceptable to the readers of the *Philosophical Magazine*.

On placing the ends of the connecting wires about two inches from their extremities, across each other, and about one-eighth of an inch apart, the usual appearance of flame took place: on gradually withdrawing the wires at this connexion the flame could be increased to one-fourth of an inch in length. In about half a minute the end of the positive wire became red-hot; it very shortly increased to a white heat, until at last it could not bear its own weight, but turned over as indicated in the figure. Considering that the effect might possibly be due to some peculiar property in the wires, they were reversed; but the effect was the same, the positive wires being invariably heated at least two inches beyond the contact, while the negative remained cool. The experiment was repeated on a future evening with the same results.



A variety of experiments were made with this battery, an account of which has since been presented to the Electrical

Society, and will be printed in the Transactions of that body.

Believe me,

My dear Sir, yours sincerely,

Clapham Common, Oct. 19, 1838.

JOHN P. GASSIOT.

Note on the preceding Communication, by Mr. Brayley.

On the 20th of October I had the pleasure of witnessing the remarkable and hitherto I believe unnoticed difference in the temperature of the positive and negative electrodes, which is described by Mr. Gassiot in the preceding communication; that gentleman having then repeated the experiment many times, in the presence of Professors Faraday and Daniell, Jonathan Pereira, Esq., F.R.S., and myself. It having been suggested that the effect might be due to the particular metal of which the electrodes were formed, Mr. Gassiot afterwards changed the copper wires usually employed, successively for wires of platinum, iron, steel, and brass, when the same phænomenon was exhibited in each instance, proving it to be due to a cause primarily connected with the activity of the pile.

LXII. *On the Composition of certain Essential Oils.* By ROBERT KANE, M.D., M.R.I.A., &c. &c. &c.*

THE analyses, which constitute the material of this paper, were undertaken as the first step in a train of research, which had for its object the solution of two problems in organic chemistry, the importance of which will be at once recognised; namely, first, whether there exists a law connecting the composition of the oils derived from the same natural family of plants? and secondly, what is the chemical nature of the essential oils as a class? is the oil of bitter almonds a type for all? Towards a decision of these questions some materials have been collected; but the investigation embraces a field so wide, that considerable time must elapse, before even such general results, as have been already obtained, can be worked into a form fit for publication. In the meantime I am induced to lay before chemists those conclusions with respect to the composition of some of the oils, which may serve as data in a solution of the first question, and as initiatory to the more abstract considerations on their composition as a class.

I. *Composition of Oil of Rosemary.*

The oil of rosemary, obtained in its purest commercial form, is colourless, and possesses strongly the odour of the

* Communicated by the Author.

fresh plant. Its specific gravity is given by Brande 0·9118. I found the oil of commerce to be 0·897; but like all these oils it differs according to seasons, from the variable quantity of stearopten it may hold dissolved. When submitted to rectification it distils over, the boiling point rising, but not rapidly; and the last portion in the retort becomes brown, and of a thick consistence. By repeated distillations it can be rendered completely pure.

Its specific gravity is then from 0·8854 to 0·8875. Berzelius states the specific gravity of the pure oil to be 0·889. It boils at a temperature from 332° to 334° Fahr. Its boiling point is almost absolutely constant,—indeed much more constant than that of any other oil I have examined. It was analysed in the ordinary way by combustion with oxide of copper, and the carbonic acid collected, by potash, in Liebig's apparatus.

| | |
|----------------------------------|---------|
| A. Material = 0·300 gramme, gave | |
| Water | = 0·315 |
| Carbonic acid | = 0·907 |
| B. Material = 0·247 gramme, gave | |
| Water | = 0·261 |
| Carbonic acid | = 0·744 |

Hence follows

| | A. | B. |
|----------|---------|-------|
| Carbon | = 83·49 | 83·31 |
| Hydrogen | = 11·66 | 11·66 |
| Oxygen | = 4·85 | 5·03 |

These analyses, having been made with quantities of oil distilled at different times, and coinciding so completely, rendered it unnecessary to increase the number.

The above result is fully expressed by the formula $C_{45}H_{38}O_2$, which gives

| | | |
|----------|---------|--------|
| C_{45} | = 276·3 | 83·63 |
| H_{38} | = 38·0 | 11·54 |
| O_2 | = 16·0 | 4·83 |
| | <hr/> | <hr/> |
| | 330·3 | 100·00 |

Evidently $C_{45}H_{38}O_2 = 9(C_5H_4) + 2HO$; that is, we may consider oil of rosemary as a hydrate of one of those numerous oils, agreeing in per cent. composition with oil of turpentine. This relation induced me to make some experiments on the action of various bodies on it, the results of which belong properly to a future memoir, but of which I shall transcribe one here.

When mixed with sulphuric acid, oil of rosemary becomes black, and neutralized by lime gives a soluble salt, which contains an acid belonging to the class composed of oxygen,

sulphur, and an organic element. If the mixture of oil of rosemary and sulphuric acid be distilled, there is obtained a fluid possessing an aromatic, yet alliaceous odour, resembling that of mesitylene. When quite pure this fluid has a specific gravity 0·8678, and boils at about 344° Fahrenheit. It gives, on analysis, the same composition as oil of turpentine; but I shall defer the details to the memoir on the Chemical Habitudes of the Oils as a Class. To this fluid I have given the name of *Rosmarine*; but I have not determined its atomic weight, so as to be able to say whether oil of rosemary be a bi-hydrate of rosmarine, and its formula $C_{45}H_{36} + 2HO$. I use the word bi-hydrate here, merely in order to represent the abstraction of 2 HO by the sulphuric acid, and not wishing to involve any consideration of theory, which will be discussed in its proper place in the memoir already alluded to.

II. Oil of Marjoram.—(*Origanum vulgare*.)

This oil is found in commerce nearly pure. Its specific gravity is variable; Mr. Brande found it 0·909; while I obtained 0·8901. This arises from the quantity of stearopten which it contains not being constant, though generally large. By rectification several times, so as to deprive it of the solid matter, it is obtained finally of the specific gravity 0·8673, the boiling point being almost completely constant at 322° Fahr. In this state it was analysed.

- A. Material = 0·368 gramme gave
 - Water = 0·368
 - Carbonic acid = 1·154
- B. Material = 0·2907 gramme gave
 - Water = 0·300
 - Carbonic acid = 0·905
- C. Material = 0·2854 gramme gave
 - Water = 0·295
 - Carbonic acid = 0·891
- D. Material = 0·2881 gramme gave
 - Water = 0·302
 - Carbonic acid = 0·898

Hence there is per cent.

| | A. | B. | C. | D. |
|----------|---------|-------|-------|-------|
| Carbon | = 86·71 | 86·08 | 86·33 | 86·18 |
| Hydrogen | = 11·11 | 11·44 | 11·44 | 11·64 |
| Oxygen | = 2·18 | 2·48 | 2·23 | 2·18 |

These results are expressible by a very simple formula, $C_{50}H_{40}O$, or $10 \{C_5H_4\} + O$, which gives

| | | |
|----------|-------|-------|
| C_{50} | = 307 | 86.48 |
| H_{40} | = 40 | 11.27 |
| O | = 8 | 2.25 |

The first analysis deviates a little from this result, in the direction opposite to that generally taken by errors of experiment in organic analysis, but so trivially, that I am not inclined to attribute to it any importance. From the remarkable relation of the formula to that of oil of turpentine, it may, perhaps, be thought that possibly the oil of origanum had the same composition, and that the two per cent. attributed to oxygen, arose from the loss consequent on an imperfect combustion; but that idea is disproved by—first, the uniformity of the analyses which were made at different times and on different quantities of oil; and secondly, by the fact that in only the fourth analysis does the hydrogen amount to what oil of turpentine should yield, that is 11.55 per cent., the general error in analysis being to give too high a value for the hydrogen. The stearopten of this oil would appear to contain much more oxygen than the oil itself; as a quantity of oil which contained some, and had consequently a boiling point of 344° , gave the following results:

| | |
|---------------|---------------------|
| Material | = 0.360 gramme gave |
| Water | = 0.350 |
| Carbonic acid | = 1.100 |

Hence per cent.

| | | |
|----------|---------|-------|
| Carbon | = 84.48 | } 100 |
| Hydrogen | = 10.80 | |
| Oxygen | = 4.72 | |

being a mixture, it admits of no formula,

III. Oil of Peppermint.

This oil had been submitted to analysis by Blanchet and Sell*, but their results having been communicated without any detail, and the analyses being exceedingly discordant, I was obliged to re-examine its constitution before I could satisfy my mind. The result has been, that I have obtained a formula differing from that of the chemists before named.

The oil of peppermint of commerce, particularly that sold as American, is often adulterated by a large quantity of oil of turpentine, from which, if once mixed, no number of rectifications can completely purify it. The rough oil, when pure, has, according to Brande, a specific gravity 0.907; I found it 0.9083. It contains a certain quantity of stearopten, though not so much as the generality of oils; when freed from this

* *Journal de Pharmacie*, vol. xx. 1834.

by repeated distillations it has a specific gravity of 0.8998, and boils between 370° and 380° Fahr.

The following are the analytical results:

| | |
|----------------------------------|---------|
| A. Material = 0.2905 gramme gave | |
| Water | = 0.318 |
| Carbonic acid | = 0.812 |
| B. Material = 0.3667 gramme gave | |
| Water | = 0.407 |
| Carbonic acid | = 1.035 |
| C. Material = 0.3443 gramme gave | |
| Water | = 0.375 |
| Carbonic acid | = 0.969 |

Hence there is per cent.

| | A. | B. | C. |
|----------|---------|-------|-------|
| Carbon | = 77.29 | 78.06 | 77.81 |
| Hydrogen | = 12.11 | 12.32 | 12.01 |
| Oxygen | = 10.60 | 9.62 | 10.18 |

The analyses A and B were made with the same specimen of oil; the correct value for carbon is therefore that of B. The analysis C was made subsequently with a different specimen.

The formula $C_{21}H_{20}O_2$ gives

| | | | |
|----------|---------|-------|-------|
| C_{21} | = 128.9 | 78.14 | } 100 |
| H_{20} | = 20.0 | 12.12 | |
| O_2 | = 16.0 | 9.74 | |

I cannot explain the difference between this result and that of Blanchet and Sell, as they give no account of the circumstances under which their analyses were made. They make no mention of having submitted their oil to rectification. Their result is $C_{12}H_{10}O$, and per cent.

| | Experiments. | | Theory. | } 100 |
|----------|--------------|-------|---------|-------|
| Carbon | = 79.63 | 79.53 | 80.35 | |
| Hydrogen | = 11.25 | 10.77 | 10.90 | |
| Oxygen | = 9.12 | 9.70 | 8.75 | |

Blanchet's result for the stearopten of oil of peppermint, though discordant with itself, yet approaches closer to mine for the oil. Thus he obtained for the solid crystalline stearopten $C_{10}H_{10}O$, and per cent.

| | Experiments. | | Mean. | Theory. | } 100 |
|----------|--------------|-------|-------|---------|-------|
| Carbon | = 79.63 | 77.27 | 78.45 | 77.28 | |
| Hydrogen | = 11.25 | 12.96 | 12.11 | 12.59 | |
| Oxygen | = 9.12 | 9.77 | 9.44 | 10.12 | |

On analysing the first portion obtained in rectifying oil of peppermint, which had a boiling point about ten degrees

lower than the general mass subsequently condensed, I obtained the following result :

Material = 0.337 gramme gave
 Water = 0.360
 Carbonic acid = 0.969

Hence per cent.

Carbon = 79.53
 Hydrogen = 11.86
 Oxygen = 8.61

This result might be considered as approximating to that of Blanchet; but this portion could not be considered as being pure oil, as it constituted not a tenth of the quantity submitted to distillation. I attributed its different composition to an adulteration by oil of turpentine, which from its greater volatility came over in the commencement. If the oil of turpentine were in large quantity, a complete rectification could not be obtained.

Upon the whole, it is evident, that we cannot consider the nature of the oil and stearopten of peppermint as being absolutely determined; at the same time, I conceive myself justified in placing some confidence in my results, from their agreement, and the care with which the substance had been purified.

IV. Oil of Pennyroyal.—(*Mentha Pulegium*.)

The oil of pennyroyal is stated by Mr. Brande to have a specific gravity of 0.939. I could not obtain it in commerce with a specific gravity higher than 0.9271. I consider, however, the specimen having that specific gravity as being pure, and that a higher density arises from dissolved stearopten. The oil of 0.9271 having been rectified, left but little solid matter; and the pure oil after distillation had a specific gravity of 0.9255. Its boiling point could not be obtained quite constant, fluctuating between 360° and 370° Fahr. At the end of this paper a few remarks on the boiling points of these oils will be found. It was analysed as follows :

A. Material = 0.3607 gramme gave
 Water = 0.353
 Carbonic acid = 1.020
 B. Material = 0.3014 gramme gave
 Water = 0.297
 Carbonic acid = 0.861

Hence per cent.

| | | | |
|----------|--------|------|---------|
| | A. | B. | |
| Carbon | = 78.2 | 79.0 | } 100.0 |
| Hydrogen | = 10.8 | 10.9 | |
| Oxygen | = 11.0 | 10.1 | |

These analyses give the formula $C_{10}H_8O$, or

| | | |
|----------|--------|--------|
| Carbon | = 61.4 | 79.30 |
| Hydrogen | = 8.0 | 10.35 |
| Oxygen | = 8.0 | 10.35 |
| | <hr/> | <hr/> |
| | 77.4 | 100.00 |

This result is exceedingly remarkable, as from it follows that oil of pennyroyal has the same composition as camphor, and as the new substance described by me in the memoir on Acetone, and which I have termed *Dumasine**. A very interesting problem, which I hope soon to be able to solve, will be, to determine the relative changes which dumasine and oil of pennyroyal undergo, in the circumstances under which common camphor yields camphoric acid.

Oil of pennyroyal is more frequently adulterated with oil of turpentine, and to a greater extent, than any other oil that I know of. I have found specimens, which delivered four-fifths of their volume of pure oil of turpentine on rectification. Thus, in the commencement of these experiments, I isolated and analysed quantities of the product of rectification of commercial oil of pennyroyal, and stated verbally at a meeting of the [Royal Irish] Academy, that I could not find any difference between rectified oil of pennyroyal and oil of turpentine. I shall detail a couple of the analyses, made under those circumstances, because the result may be used as a test for the closeness of the other experimental results, and for the legitimacy of the formulæ deduced from them.

Fluid obtained by rectifying impure oil of pennyroyal, specific gravity = 0.8673; boils constant at 315° .

A. Material = 0.2183 gramme gave

Water = 0.232

Carbonic acid = 0.697

B. Material = 0.2433 gramme gave

Water = 0.260

Carbonic acid = 0.779

Hence

| | A. | B. | Theory. |
|----------|---------|-------|---------|
| Carbon | = 88.29 | 88.56 | 88.45 |
| Hydrogen | = 11.78 | 11.87 | 11.55 |

Its formula C_5H_4 . It was oil of turpentine.

* It had been my intention to insert the account of the composition and properties of *Dumasine* as an appendix to the memoir on Pyroacetic Spirit, but it has been withheld in order, when further studied, to form the subject of an independent paper. A notice of its discovery and composition was published in the Proceedings of the Royal Irish Academy, No. IV. page 61, and copied from thence into the Philosophical Magazine for 1838. (Lond. and Ed. Phil. Mag. vol. xii. p. 108.)

V. Oil of Spearmint.—(*Mentha viridis*.)

The oil of spearmint is stated by Brande to have a specific gravity 0.939. That which I found in commerce had, in the rough state, a specific gravity 0.9142, and when freed by distillation of a considerable quantity of stearopten, 0.876. It boiled pretty constantly at 320° Fahr. It gave by analysis as follows:

A. Material = 0.3087 gramme gave

Water = 0.311

Carbonic acid = 0.954

B. Material = 0.350 gramme gave

Water = 0.350

Carbonic acid = 1.075

C. Material = 0.4284 gramme gave

Water = 0.439

Carbonic acid = 1.327

Hence the per cent. composition:

| | A. | B. | C. | |
|----------|---------|-------|-------|---------|
| Carbon | = 85.44 | 84.94 | 85.66 | } 100.0 |
| Hydrogen | = 11.19 | 11.11 | 11.38 | |
| Oxygen | = 3.36 | 3.95 | 2.96 | |

These results give the formula $C_{35}H_{28}O$; that is,

| | | | |
|----------|-------------|-------------|---------|
| C_{35} | = 214.9 | 85.67 | } 100.0 |
| H_{28} | = 28.0 | 11.15 | |
| O | = 8.0 | 3.18 | |
| | <hr/> 250.9 | <hr/> 100.0 | |

This formula, like that of oil of origanum, may be represented by a multiple of oil of turpentine + oxygen = $7\{C_5H_4\} + O$. It is however impossible to make any deduction from these facts with respect to its nature.

VI. Oil of Lavender.—(*Lavandula spica*.)

The oil of lavender is remarkable for containing, probably, more stearopten than any other essential oil, the quantity itself being variable with the season and with the climate. Thus, in the oil from the south of Europe (Murcia), it is said to amount to more than one-half its weight, and I have often found a fourth of the English oil to remain behind on distillation, so loaded with stearopten as to become quite solid on cooling. These circumstances,—the quantity of solid matter, and its not differing very much in volatility from the oil,—present great obstacles to the obtaining oil of lavender in an absolutely pure condition.

The oil examined by Mr. Brande, and which probably contained much stearopten, was found by him of a specific gravity 0·9206. In the specimen which I tried the specific gravity was 0·9174. By repeated rectifications, by which as much of the solid matter as possible was separated, it was obtained 0·8865, and then boiled at 370° Fahr. Another portion equally rectified had a specific gravity 0·8745, and boiled at 365° Fahr. Saussure, who likewise examined this oil, states its specific gravity, when pure, to be 0·877.

A specimen of the commercial oil, the specific gravity of which was 0·9172, was rectified, and the product received in four different vessels, stopping when the residue in the retort appeared to become thick.

| | | |
|-------|--------------------------|--|
| No. 1 | had sp. gr. = 0·8864 | } All boiled between 360°
and 370°. |
| 2 | = 0·8879 | |
| 3 | = 0·8884 | |
| 4 | = 0·9050 boiled at 390°. | |

The residue solidified by cooling. The difficulty of pronouncing on the absolute purity of any one portion of oil of lavender is thus evident.

The analysis of this oil was conducted in the usual way. Different portions of oil were selected, in consequence of their boiling points and specific gravities not being in accordance.

A. Specimen sp. gr. = 0·8865; boiling point = 370° F.

Material = 0·4333 gramme gave

Water = 0·441

Carbonic acid = 1·245

B. Specimen sp. gr. = 0·8745; boiling point 365°.

Material = 0·2835 gramme gave

Water = 0·299

Carbonic acid = 0·819

C. Specimen sp. gr. = 0·8864; boils at from 365° to 370°.

Material = 0·3788 gramme gave

Water = 0·394

Carbonic acid = 1·079

Hence the composition

| | A. | B. | C. |
|----------|---------|-------|-------|
| Carbon | = 79·45 | 75·77 | 78·81 |
| Hydrogen | = 11·30 | 11·73 | 11·55 |
| Oxygen | = 9·25 | 12·50 | 9·64 |

As it is quite evident that, the heavier this oil is, the more stearopten it contains, and consequently, the less pure it is; and as, by Dumas's analysis, stearopten, if not completely identical, has at least the same composition with common

camphor, I am inclined to consider the high values for carbon in the oils used in analyses A and C as owing in great part to its presence, and to assign the analysis B as a closer approximation to the composition of the pure oil. With this idea agrees Saussure's old result, who from an oil of the sp. gr. 0.877 obtained the composition

| | | |
|----------|---------|---------|
| Carbon | = 75.50 | } 100.0 |
| Hydrogen | = 11.07 | |
| Oxygen | = 13.07 | |
| Nitrogen | = 0.36 | |

The nitrogen being now acknowledged to result from imperfections in the method employed, the tendency of which was also generally to give an under-estimate for the amount of hydrogen, his result is found to agree with that of analysis B; but whether from both oils being pure, or from both being equally impure, I cannot undertake to say.

Under such circumstances it is scarcely useful to attempt the construction of a formula, as representing the result obtained. $C_{15} H_{14} O_2 = 3 C_5 H_4 + 2 HO$, may, however, be employed:

| | | | | |
|-------|----------|---|-------------|-------------|
| Thus, | C_{15} | = | 92.1 | 75.5 |
| | H_{14} | = | 14.0 | 11.5 |
| | O_2 | = | 16.0 | 13.0 |
| | | | <hr/> 122.1 | <hr/> 100.0 |

I must not be understood as stating positively this formula to represent the truth.

Additional Remarks.—There is a peculiarity in the method of ebullition of these oils which renders it very difficult to fix upon a certain fixed temperature as the boiling point, but rather compels us to consider the oil as boiling within a limit of temperature, sometimes extending to ten degrees of Fahrenheit. Thus, in taking the boiling point of an oil in a tube, a thermometer being immersed therein to some distance above the bulb, the oil will enter into full ebullition apparently at 355° , and the temperature of the thermometer, on continuing the boiling for five or ten minutes, will gradually rise to 360° or 365° , and will not then stop so completely, but that an ebullition continued for five or six minutes more may produce a further rise of a couple of degrees. If the oil be allowed to cool, and be then again heated, the same phenomenon will be repeated, and so, as often as may be wished; but the most colourless oil, when thus frequently heated, gradually becomes brown, and then there is a permanent elevation of the boiling point, arising from decomposition.

I attribute this phenomenon to an unequal distribution of heat through the mass, and to the heat being supplied by the spirit-lamp too rapidly to be carried off from the oil by the vapour formed at the limited surface of contact of the oil with the air in the tube. This is supported by the fact, that by moderating the heat the boiling point may be kept constant; but, by a suitable heat, it may be kept constant at any degree, between the limits already alluded to. This is the reason why the boiling points of the oils analysed are generally given within a limit of a few degrees. Some cases where the boiling points were almost really constant, I attribute to a closer approach to absolute purity in the oil.

A great deficiency exists in analytical results obtained under circumstances such as those described in the present paper, from the total want of a control over their exactitude; and one or two words on the nature of these controlling results, with reference to obtaining such in our experiments, may be here of use. There are four modes of control—1st, by synthesis, which is the most complete, but which in organic chemistry is attainable only in some very few cases. Of these few, the synthesis of urea, and that of acetic acid, may be taken as illustrations. 2nd, The method next in completeness is the breaking up of the body into other compounds whose composition is already known; as, formal into formic acid and methylic alcohol; acetal into acetic acid and vinic alcohol: acetic acid into carbonic acid and acetone. 3rd, The determination of the atomic weight of the body, by the compounds into which it enters with other well-known substances. Thus the composition of oxalic æther controls the analysis of sulphuric æther, and the salts of the vegetable alkaloids give the only means of verifying the composition of the base itself. The fourth method of control is limited to such bodies as pass into vapour without being decomposed, and then the density of the vapour should stand in some simple relation to the sum of the densities of the constituents, taken in the atomic proportions given by analysis. Thus the discussion as to whether naphthaline was represented by the formula $C_5 H_2$ or $C_8 H_3$ was decided by the vapour possessing a density immediately following from the former, but inconsistent with the latter. That, however, we must not insist on very simple relations, is shown by the complex numbers found for some of the inorganic compounds by Mitscherlich and Dumas.

Now in examining the composition of the oils, we are debarred from efficiently exerting any of these methods of control:—1st, we cannot generate them by synthesis; 2nd, we do not as yet know their chemical nature sufficiently to break them

up into other bodies with which we can get more definite results; and 3rd, their combinations with other bodies have not been as yet developed. Towards the application of these methods I have made some progress in the cases of oils of rosemary, lavender, and oil of turpentine, which all give with sulphuric acid and a base, soluble salts, of which that from turpentine alone has been completely analysed. The atomic weight of turpentine from the salt of lime is found to be $C_{30}H_{16}$; the same as from artificial camphor; and the salt has the composition $SO_3 \cdot CaO + C_{30}H_{16}$, belonging to a series distinct from the sulpho-vinates on the one hand, and the sulpho-naphthalates upon the other, and being probably an analogue to the sulpho-mesitylic acid described in my memoir upon Acetone. I mention these results, although they properly belong to a different paper, in order to point out the probable means of applying the methods of control to the essential oils in future investigations. I attempted very often to determine the densities of the vapours of the essential oils with a bath of chloride of zinc, but I never obtained a result on which I could with satisfaction rely. The residual oil was evidently altered in its nature and appearance, and there always remained in the globe a certain quantity of permanent gas. I attribute the imperfect success of these attempts, to the mutual action of the oil and air of the globe at the high temperature necessary for the experiment; but by a modification of the apparatus I may possibly at a future period succeed.

A circumstance connected with the purification of the oils employed in the analyses detailed in this paper requires some notice here, as it has not been adverted to under the special heads; namely, the means used to secure the perfect freedom of the oils from water. This is the more important, as particularly in the results obtained with the oils of marjoram and spearmint, the small quantity of oxygen might be conceived as being derivable from this source, unless proper precaution had been taken. The oils, previous to rectification, were in all cases digested for several days on recently fused chloride of calcium; then poured off, and distilled; and the portions selected for analysis allowed to remain in contact for about twelve hours with a few pure fragments of chloride of calcium, before being used. In no case did the oil appear to act on, or dissolve any of the fused salt; and hence it was only necessary to pour the oil off from the chloride, and not to again distil it, in order to obtain it pure.

LXIII. *On the Motion and Rest of Fluids.* By J. J. SYLVESTER, *Professor of Natural Philosophy in University College, London.*

MOSTROGRADSKY's memoir on this subject inserted in the Scientific Memoirs seems to have excited much attention, and has been made the occasion of some annotations by a distinguished writer in the Philosophical Magazine. Mr. Ivory's recent papers in the same periodical must still more tend to invest with a new interest all such speculations. It seems to me desirable therefore to present the theory of fluids in all the simplicity of which it is susceptible.

I consider a fluid as a collection of particles subject to some law of relative position other than that of rigidity. These particles by their mutual actions maintain the connexions of the system. As to the law of force between them we know nothing; but I assume it is a general principle of nature, that for each instant of time the sum of the internal actions (reckoned by the product of each particle into the square of the space due to the internal force acting on it) is a minimum. This in fact is Gauss's principle of least restraint. We may if we please split this principle into two parts; that is to say, assume that the internal system of forces is always such as if acting alone would keep the fluid at rest; and then again assume that any equilibrating system of forces must be subject to the law of virtual velocities. I say *assume*, because it is impossible *à priori* to prove this.

Lagrange's so-called demonstration is unworthy of his name, and (albeit sanctioned by the powerful oral authority of an ex-Cambridge Professor) contrary alike to sense and honesty. It is better therefore at once to proceed upon Gauss's principle. It might easily be shown that this is in effect tantamount in all cases to D'Alembert's and Lagrange's principles combined.

Before entering upon the investigation I may call attention to one point of great analytical interest, and relating to the difficult subject of the algebraical sign, viz. that if the density of a point (x, y) in any circumscribed space be expressed by the

quantity $\frac{du}{dx} + \frac{dv}{dy}$ so that the mass is

$$\iint dx dy \left(\frac{du}{dx} \right) + \iint dx \cdot dy \cdot \frac{dv}{dy},$$

that is not equivalent to

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$\int (u dy + v dx)$, i.e. if we please $\int \left(u \frac{dy}{ds} + v \frac{dx}{ds} \right) ds$

(where s is for clearness' sake and to avoid double limits taken an element of the bounding curve) as at first sight it might appear to be, but is in fact equal to

$$\int \left(u \cdot \frac{dy}{ds} - v \frac{dx}{ds} \right) ds.$$

I shall demonstrate this point in the next number of the Magazine. It at first caused me some trouble in conducting the annexed inquiry. I shall also take occasion at some other time to revert to a new species (as I believe) of partial differential equations; that is to say, where there are fewer of them than of the principal variables, which may be called therefore Indeterminate Partial Differential Equations. A complete solution of one of these appears in the subjoined

Investigation.

For the sake of simplicity I take an incompressible fluid. The method is nowise different for a fluid of varying density.

Let $\Delta x \Delta y \Delta z$ be any displacement undergone by a particle at the point x, y, z parallel to the axes x, y, z respectively; it is easily shown that to satisfy the condition of invariability of mass we must have

$$\frac{d \Delta x}{d x} + \frac{d \Delta y}{d y} + \frac{d \Delta z}{d z} = 0. \quad \dots\dots\dots (a.)$$

One relation between u, v, w the velocities parallel to x, y, z is obtained immediately by putting $u \delta t, v \delta t, w \delta t$ for $\Delta x, \Delta y, \Delta z$, which gives

$$\frac{d u}{d x} + \frac{d v}{d y} + \frac{d w}{d z} = 0 \quad \dots\dots\dots (1.)$$

as usual.

Again, if $X Y Z$ be the impressed forces, and $X_i Y_i Z_i$ the internal forces acting on any particle parallel to the axes, we have

$$X_i + X = \frac{d u}{d t} + \frac{d u}{d x} \cdot u + \frac{d u}{d y} \cdot v + \frac{d u}{d z} \cdot w \quad \dots \quad (2.)$$

$$Y_i + Y = \frac{d v}{d t} + \frac{d v}{d x} \cdot u + \frac{d v}{d y} \cdot v + \frac{d v}{d z} \cdot w \quad \dots \quad (3.)$$

$$Z_i + Z = \frac{d w}{d t} + \frac{d w}{d x} \cdot u + \frac{d w}{d y} \cdot v + \frac{d w}{d z} \cdot w \quad \dots \quad (4.)$$

from the mere geometry of the question.

Finally, Gauss's principle teaches us that

$$\iiint dx \cdot dy \cdot dz \cdot \{X_i \Delta X_i + Y_i \Delta Y_i + Z_i \Delta Z_i\} = 0 \dots (\beta.)$$

Now

$$\begin{aligned} & \frac{d(X + X_i)}{dx} + \frac{d(Y + Y_i)}{dy} + \frac{d(Z + Z_i)}{dz} \\ & \left(\frac{du}{dx}\right)^2 + \left(\frac{dv}{dy}\right)^2 + \left(\frac{dw}{dz}\right)^2 \\ & + 2 \cdot \left\{ \frac{dv}{dz} \cdot \frac{dw}{dy} + \frac{dw}{dx} \cdot \frac{du}{dz} + \frac{du}{dy} \cdot \frac{dv}{dz} \right\} \end{aligned}$$

as appears from the =^{ns} (1) (2) (3) (4), and hence

$$\frac{d \Delta X_i}{dx} + \frac{d \Delta Y_i}{dy} + \frac{d \Delta Z_i}{dz} = 0.$$

The complete solution of which, free from the sign of integration, is $\Delta X_i \Delta Y_i \Delta Z_i$, being subject to no other restrictions than such as are imposed by this equation

$$\Delta X_i = \frac{d\psi}{dy} - \frac{d\phi}{dz}$$

$$\Delta Y_i = \frac{d\omega}{dz} - \frac{d\psi}{dx}$$

$$\Delta Z_i = \frac{d\phi}{dx} - \frac{d\omega}{dy}$$

ω, ϕ, ψ being any three independent functions of x, y, z .

On substituting these values in =ⁿ (β) we obtain

$$\begin{aligned} & \iiint dx \, dy \, dz \cdot \left\{ X_i \frac{d\psi}{dy} - Y_i \frac{d\psi}{dx} \right\} \\ & + \iiint dx \, dy \, dz \cdot \left\{ Y_i \frac{d\omega}{dz} - Z_i \frac{d\omega}{dy} \right\} \\ & + \iiint dx \, dy \, dz \cdot \left\{ Z_i \frac{d\phi}{dx} - X_i \frac{d\phi}{dz} \right\} = 0. \end{aligned}$$

This may be put under the form

$$\begin{aligned} & \int dz \iint dx \, dy \cdot \left\{ \frac{d}{dy} (\psi X_i) - \frac{d}{dx} (\psi Y_i) \right\} \\ & + \int dx \iint dy \, dz \cdot \left\{ \frac{d}{dz} (\omega Y_i) - \frac{d}{dy} (\omega Z_i) \right\} \\ & + \int dy \iint dz \, dx \cdot \left\{ \frac{d}{dx} (\phi Z_i) - \frac{d}{dz} (\phi X_i) \right\} \end{aligned}$$

$$\begin{aligned}
& -\iiint dx dy dz \cdot \psi \left(\frac{dX_l}{dy} - \frac{dY_l}{dx} \right) \\
& -\iiint dx dy dz \cdot \omega \left(\frac{dY_l}{dz} - \frac{dZ_l}{dy} \right) \\
& -\iiint dx dy dz \cdot \phi \left(\frac{dZ_l}{dx} - \frac{dX_l}{dz} \right) = 0.
\end{aligned}$$

Here it must be remembered that ω , ϕ , ψ are perfectly independent of each other. Also the values of the three first written quantities depend upon the values of X_l , Y_l , Z_l at the bounding surface; the values of the three last-written depend upon the general values of X_l , Y_l , Z_l . It is clear therefore that each system of three equations and each member of each system must be separately zero.

The three latter equations give

$$\left. \begin{aligned}
\frac{dX_l}{dy} - \frac{dY_l}{dx} &= 0 \\
\frac{dY_l}{dz} - \frac{dZ_l}{dy} &= 0 \\
\frac{dZ_l}{dx} - \frac{dX_l}{dz} &= 0.
\end{aligned} \right\} \dots\dots\dots (\gamma.)$$

The three former require that for each section of the surface parallel to the plane xy

$$\left. \begin{aligned}
& \int \psi (X_l dx + Y_l dy) = 0 \\
& \text{for each section parallel to } yz, \\
& \int \omega (Y_l dy + Z_l dz) = 0 \\
& \text{for each section parallel to } zx, \\
& \int \phi (Z_l dz + X_l dx) = 0.
\end{aligned} \right\} \dots\dots (\delta.)$$

and these equations are to hold good whatever ψ , ϕ , ω may be. From the equations (γ .) we derive

$$X_l dx + Y_l dy + Z_l dz = d\phi \quad \dots \quad (5.)$$

from equations (δ .) we obtain

ϕ = constant for all points in any section of the bounding surface parallel to the plane of xy

ϕ = constant for all points in any section of the bounding surface parallel to the plane of yz

ϕ = constant for all points in any section of the bounding surface parallel to the plane of zx .

* See remark at Introduction.

Now by drawing through all the points in a plane parallel to xy , planes parallel to yz , we may cover the whole surface; hence ϕ is constant all over the surface bounding the fluid.

$\therefore X_1 dx + Y_1 dy + Z_1 dz = 0$ for all variations of dx, dy, dz taken upon the surface \dots (6.)

The equations 1, 2, 3, 4, 5, 6 are coincident with those obtained by the usual method; with this difference, that X_1, Y_1, Z_1 here take the place of $-\frac{dp}{dx} - \frac{dp}{dy} - \frac{dp}{dz}$.

Thus then we have obtained all the conditions requisite for determining the motion of fluids from the universal principle of least constraint conjoined with the specific character of the system in question.

General Remarks.

In the case of equilibrium, i. e. in the case where no particle moves, we have $X_1 + X = 0, Y_1 + Y = 0, Z_1 + Z = 0$. Hence $X dx + Y dy + Z dz$ is a complete differential always and zero for the surface.

The above results have been obtained upon the principles of the differential calculus, and the continuity of the forces has been tacitly assumed. If now we were to suppose forces of finite magnitude (as compared with the *whole sum* acting upon the entire system) to be applied to a layer of single particles or to a layer of a thickness of the same order of magnitude as the distances between the particles themselves, (which has been treated as an infinitesimal) it would appear that our results would be no longer applicable, just in the same manner as it would be erroneous to apply the principle of *vis-viva* (for example) without modification, to the case of impulsive forces, because we had deduced it by the calculus in the case of the motion being continuous. Hence the above equations ought not strictly to apply to the motion or rest of a fluid contained between physical surfaces; for the pressure afforded by these surfaces, whatever its actual value may be, we know *a priori* is commensurable with the whole amount of force acting on the fluid; but the immediate application of this pressure (alias repulsive force) is confined to the bounding layer of fluid particles, or at most extends to a distance bearing a low ratio to the distances between the particles themselves.

According to the non-applicability of the equations for free fluids to the case of fluids confined at the boundaries and to an independent investigation upon the minimum principle for this class of problems, it is that I look for the true explanation of the phenomena of capillary attraction (vulgarly so called).

LXIV. *On an extension of Sir John Wilson's Theorem to all numbers whatever.* By J. J. SYLVESTER, *Professor of Natural Philosophy in University College, London.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE annexed original theorem in numbers will serve as a pendant to the elegant discovery announced by the ever-to-be-lamented and commemorated Horner*, with his dying voice, in your valued pages.

THEOREM.

If N be any number whatever and

$$p_1, p_2, p_3 \dots p_c$$

be all the numbers less than N and prime to it, then either

$$p_1 \cdot p_2 \cdot p_3 \dots p_c + 1$$

$$\text{or else } p_1 \cdot p_2 \cdot p_3 \dots p_c - 1$$

is a multiple of N .

Yours with high respect,

University College, London, Oct. 22, 1838.

J. J. SYLVESTER.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 21.—The following papers were read, viz.

“On the structure of the teeth, the vascularity of those organs, and their relation to bone.” By John Tomes, Esq. Communicated by Thomas Bell, Esq., F.R.S., Professor of Zoology in King's College, London.

The microscopical examinations which the author has made of the structure of the teeth of man and various animals, lead him to the conclusion that their bony portions are formed of minute tubes, disposed in a radiated arrangement, in lines proceeding everywhere perpendicularly from the inner surface of the cavity containing the pulp. These tubuli are surrounded by a transparent material, which cements them together into a solid and dense mass. He finds, by applying the test of muriatic acid, that carbonate as well as phosphate of lime enters into their composition. In man, the tubuli, during their divergence from their origin at the surface of the central cavity, send off a number of very minute fibrils; and on approaching the enamel or the granular substance, which cover respectively the crown and the fangs of the tooth, the tubuli divide into smaller

* Horner's proof is highly valuable as a novel and highly ingenious form of reasoning, but his theorem may be deduced with infinitely more ease and brevity from Fermat's than he seems to have been aware of.

ones, which freely anastomose with one another, and then either are continued into the enamel, or terminate at the boundary between these two substances. Various modifications of this structure, exhibited in the teeth of different animals, in the class Mammalia and Fishes more particularly, are minutely described. The granular substance appears to be composed of irregularly shaped osseous granules, imbedded in the same kind of transparent medium which cements the tubuli together. External to the granular portion, the author finds another substance entering into the formation of the simple tooth, and commencing where the enamel terminates; and which he describes as beginning by a thin and transparent layer containing only a few dark fibres, which pass directly outwards; but assuming, as it proceeds towards the apex of the fang, greater thickness and opacity, and being traversed by vessels.

External to the enamel, and in close connexion with it, in compound teeth, is situated the *crusta petrosa*, a substance very similar to the bony layer of the simple tooth. It contains numerous corpuscles, and is traversed by numerous vessels entering it from without, and anastomosing freely with one another, but terminating in its substance. These investigations of the structure of the different component parts of teeth, furnish abundant evidence of their vascularity and consequent vitality.

"On the evolution of Nitrogen during the growth of plants, and the sources from whence they derive that element." By Robert Rigg, Esq. Communicated by the Rev. J. B. Reade, M.A., F.R.S., &c.

In this communication the author follows up his inquiry into the influence and importance of nitrogen in vegetable physiology, by noticing, in the first place, the experiments of Dr. Daubeny, M. De Saussure, Sir Humphry Davy, and those which he himself has made; all of which tend to prove that nitrogen is evolved during the healthy performance of the functions of plants; that the proportion which it bears to the oxygen given off is influenced by the sun's rays; but that owing to the necessary exclusion of the external atmosphere during the progress of the experiments, it is impossible, with any degree of accuracy, to calculate the volume of these evolved gases during any period of the growth of plants in their natural state.

If to this indefinite quantity of nitrogen given off by plants there be added that definite volume incorporated into their substance and shown in the author's former tables*, the question arises, whence do plants derive their nitrogen, and does any part of it proceed from the atmosphere? A problem which the author proposes to solve by a series of tabulated experiments upon seeds, and seedling plants, indicating a large excess of nitrogen in the latter, and under such circumstances of growth that he is compelled to fix upon the atmosphere as its source.

By the same mode of experimenting, the author attempts to show that the differences which we find in the germination of seeds and the growth of plants in the shade and sunshine, are apparently due in a great measure to the influence of nitrogen. And he concludes

* See the present volume, p. 223.

by observing, that he does not touch upon the practical application of the subject wherein the real value of the inquiry consists; it is his object to draw attention to an element which, though in some instances so minute in quantity as to be with difficulty detected in our balances, has nevertheless been wisely assigned to discharge the most important functions.

“On the decussation of fibres at the junction of the Medulla Spinalis with the Medulla Oblongata.” By John Hilton, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author first alludes to what usually happens in affections of the brain, namely, that the loss of voluntary power and of sensation manifest themselves in the opposite side of the body to that in which the cerebral lesion exists, a fact which has been attempted to be explained by the crossing of the fibres at the junction of the *medulla oblongata* with the anterior or motor columns of the *medulla spinalis*; but such a structure, he observes, affords no explanation of the loss of sensation. The author then, referring to the communication of Sir Charles Bell to the Royal Society, in the year 1835*, describing a decussation connected with the posterior columns, or columns of sensation, mentions that the accuracy of these dissections was doubted by Mr. Mayo and other eminent anatomists. The author proceeds to state that the symptoms of cerebral lesion do not always take place on the opposite side of the body to that in which the lesion of the brain exists, but that they occur sometimes on the same side; that the loss of power and of sensation, although confined to the same side, may exist in either the upper or the lower extremity; but that both are not necessarily implicated; and that, in fact, cases occur where there are marked deviations from what may be considered the more common occurrence. Having observed such cases, and not being aware of any satisfactory explanation, the author examined with care the continuation upwards of the anterior and posterior columns of the spinal marrow into the *medulla oblongata*, and found that the decussation at the upper part of the spinal marrow belonged in part to the columns for motion, and in part to the columns for sensation; and farther, that the decussation is only partial with respect to either of these columns; thus elucidating by the observation of the actual structure what before appeared very unsatisfactory in pathology, and anomalous in disease.

The paper is illustrated by drawings made from the dissections of the author.

“Description of a self-registering Thermometer and Barometer invented by the late James Coggan, Esq., and bequeathed by him to the Royal Society.” By Roderick Impey Murchison, Esq., F.R.S., V.P.G.S., &c.

The self-registering thermometer used by Mr. Coggan is of Six's construction, and consists of a siphon tube, open at one extremity, and operating by the expansion and contraction of a large body of spirit pressing on a column of mercury in the lower bend of the tube.

* See Lond. and Edinb. Phil. Mag., vol. vii. p. 138.

On the other side of the wooden frame to which this thermometer is fixed, a siphon barometer is attached ; and both these instruments are made to act on iron-floats suspended by a thread, and counterpoised over a pulley. Transverse wires are affixed to these threads, and are forced against a sheet of ruled paper on a frame, which from its connexion with a clock is advanced a certain space each day, by a spring hammer forming part of the striking machinery of the clock.

" On the action of light upon the colour of the River Sponge." By John Hogg, M.A., F.L.S., C.P.S., &c., Fellow of St. Peter's College, Cambridge. Communicated by Thomas Bell, Esq., F.R.S.

The author found that the green colour of the *Spongilla fluviatilis*, or river sponge, is acquired solely through the agency of light, and is lost when the sponge is removed from its influence. As this does not appear to be the case with *Actiniæ*, the *Hydra viridis*, or any other Polype, the author is disposed to consider this production as being nearer allied to the Algæ or Fungi, than to any tribe belonging to the animal kingdom*.

" Researches on the Tides. Ninth Series. On the deduction of the Laws of the Tides, from short Series of Observations." By the Rev. W. Whewell, M.A. Trin. Coll., Cambridge†.

It is very desirable to ascertain whether it is possible to deduce the laws of the tides from short series of observations; since, if it be so, not only does the construction of good tide tables for different places become more easy; but also the value of tide tables is much increased, if the predicted tides agree with those of each year as well as with the mean of many years. The object of the author in the present paper is to determine this point by the discussion of several years' observations of the tides at Plymouth and at Bristol. The calculations for the former place were executed by Mr. Dessiou and Mr. Ross in the Hydrographer's Office at the Admiralty; the calculations for Bristol were performed by Mr. Bunt, in virtue of a grant of money from the British Association. The result of these discussions is, that a very regular form and good approximation for the semimenstrual inequality may be obtained from the observations of one year; that the existence of the lunar parallax corrections appears very clearly in the observations of one year; and that its value may be determined from a series of three or four years. The lunar declination corrections are more irregularly given by short series of observations; but in a series of four or five years, the general form and approximate value of the corrections become manifest. In the course of these calculations such questions as the following were proposed, and their solution attempted: 1. To which transit of the moon ought we to refer the tide? It appears that the transit which produces the best accordance with theory, is that which Mr. Lubbock terms transit B, which is an epoch about 42 hours anterior to the high water at Bristol and Plymouth. 2. How does a change of

* [Mr. Gray arrived at the same conclusion, but on physiological grounds, some years since. See Zoological Journal, vol. i. p. 50.—EDIT.]

† See Lond. and Edinb. Phil. Mag. vol. xii. p. 354. note.

the epoch affect, first, the semimenstrual inequality; secondly, the parallax correction of the time; thirdly, the declination correction of the times; fourthly, the parallax correction of heights; and fifthly, the declination correction of the heights? 3. Does the parallax corrections of height vary as the parallax? 4. Does the parallax correction of time vary as the parallax? 5. Does the declination correction of the heights vary as the square of the declination? 6. Does the declination correction of time vary as the square of the declination? 7. Can the laws of the corrections be deduced from a single year? 8. Are there any regular differences between the corrections of successive years? 9. Do the corrections at different places agree in themselves? It does not appear that any change of the epoch will produce an accordance of the observed laws with the theory, some of the inequalities requiring one epoch for this purpose, and some requiring another. The inequalities in different years and different places are also compared.

Mr. Whewell remarks, that since it has now been shown that good tide tables may be obtained from short series of observations, his researches with regard to the determination of the lunar corrections may be concluded; and the proper mode of farther prosecuting the subject, would be to have tide observations at several stations, each observatory reducing its own observations, and thus constantly improving the tables, as is practised in other branches of Astronomy.

“Researches in Embryology.” *First Series.* By Martin Barry, M.D., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Communicated by P. M. Roget, M.D., Sec. R.S.

This paper is divided into two parts. In the first part the author describes the origin and structure of the ovisac, a vesicle common to all vertebrated animals, but hitherto regarded as the inner membrane of the “folliculus Graafianus” in Mammalia, and by some authors denominated the “chorion” in other Vertebrata. He also describes the real nature of the “folliculus Graafianus,” and its relation to the calyx of the Bird; the germinal vesicle and its contents, as being the most primitive portion of the ovum; the order of formation of the several other parts of the ovarian ovum; and the true chorion of Mammalia as being a structure superadded within the ovary.

In the second part the author describes a granulous tunic of the ovum of Mammalia not hitherto observed; the manner of origin of the “membrana granulosa” of authors; the different situations of the ovum in the Graafian vesicle at certain periods *ante coitum*, not hitherto observed; and certain structures by means of which the ovum is made to occupy these several situations.

The following are the principal facts made known by Dr. Barry in this memoir; but other facts are also mentioned, which he intends to make the subject of a future communication. In Mammalia and in Birds the germinal vesicle and its contents are those parts of the ovum which are first formed. The germinal vesicle at an early period is surrounded by peculiar granules, forming an envelope

not hitherto described. The ovum of all vertebrated animals is contained in a vesicle (the "chorion" of some authors, found in Birds, Amphibia, and Fishes), which is essentially the same in structure wherever found, and which he thinks it desirable universally to denominate an *ovisac*. This vesicle is the "*couche interne*" of the Graafian vesicle, as described by Professor Baer. The Graafian vesicle of Mammalia is nothing more than an *ovisac* that has acquired a covering or tunic, susceptible of becoming highly vascular, which covering is the "*couche externe*" of the Graafian vesicle as described by Baer. The *ovisac* of Birds, Amphibia, and Fishes ("chorion" of some authors), acquires in like manner a covering or tunic, susceptible of becoming highly vascular; and by the union of the *ovisac* with this covering, there is constituted a structure analogous to the Graafian vesicle of Mammalia. The quantity of yelk in the former being large, that portion of the ovary which contains the structure here referred to (as analogous to the Graafian vesicle of Mammals) becomes pendent; and now the united coverings of the yelk-ball,—viz. the *ovisac*, its external tunic, the ovarian stroma, and the peritoneal investment,—are together called the *calyx*. From this it will be obvious that the Graafian vesicle is not a structure peculiar to Mammalia, as it has been supposed.

The *ovisac* has at first an elliptical or ellipsoidal form, becomes more spherical, and in Mammalia is often met with somewhat tapered at one end. The structure of the *ovisac* in some of the Mammalia may be examined when it does not exceed in length the 50th or even the 100th part of a Paris line, that is, in the latter case, the 1125th of an English inch. Myriads of *ovisacs* with their contents are formed that never reach maturity. Some of the *ovisacs* which do not reach maturity are situated in the parietes of Graafian vesicles in Mammalia, or of the corresponding structures in other Vertebrata; being sometimes formed in this situation, and sometimes included within the covering which the larger *ovisac* acquires. The minute *ovisacs* so situated the author proposes to denominate *parasitic* *ovisacs*. The *ovisac* is often found in a cavity proper to itself, with the walls of which it has no organic union. The granules forming the envelope of the germinal vesicle above referred to, and subsequently found in the fluid of the *ovisac*, are very peculiar in their appearance, contain a nucleus, and sometimes also a pellucid fluid, and are intimately connected with the evolution of the ovum. These granules are present in largest quantity in the *ovisac* of Mammalia; yet granules essentially the same exist in an early stage in the *ovisac* of Birds, and are sometimes met with in that of Fishes.

A continual disappearance of ova, and a formation of others, are observable even at a very early age. The ovum of Mammalia when completely formed is at first situated in the *centre* of the *ovisac*. It is at this period supported in the centre of the *ovisac* by an equable diffusion of granules throughout the fluid of the latter. The *ovisac* about the same time begins to acquire a covering or tunic,

by which addition, as already stated, there is constituted a Graafian vesicle; and of the latter, the ovisac is now the inner membrane. After this period, then, it is proper to speak, not of an ovisac, but of a Graafian vesicle. The peculiar granules of the Graafian vesicle arrange themselves to form three structures, viz. the *membrana granulosa* of authors, and two structures not hitherto described, one of which the author proposes to name the *tunica granulosa*, and the other, which is rather an assemblage of structures than a single structure, the *retinacula*. The *tunica granulosa* is a spherical covering proper to the ovum, and its presence explains why the outer line in the double contour of the thick chorion has remained so long unobserved. At a certain period this tunic, in some animals at least, is seen to have tail-like appendages, consisting of granules similar to its own. The *retinacula* consist of a central mass containing the ovum in its *tunica granulosa*, and of cords or bands extending from this central mass to the *membrana granulosa*. These structures at a certain period became invested by a membrane. The offices of the *retinacula* appear to be,—first, to suspend the ovum in the fluid of the Graafian vesicle,—next, to convey it to a certain part of the periphery of this vesicle,—and subsequently to retain it in the latter situation, and also to promote its expulsion from the ovary. The particular part of the periphery of the Graafian vesicle to which the ovum is conveyed, is uniformly that directed towards the surface of the ovary. The mass of granules escaping with the ovum on the bursting of a Graafian vesicle under the compressor, is composed chiefly of the *tunica granulosa* and the ruptured *retinacula*. The “cumulus” of Professor Baer is made up of the parts called by Dr. Barry the *tunica granulosa* and the central portion of the *retinacula*; and the band-like portions, collectively, of what Dr. Barry calls the *retinacula*, mainly contribute to produce the appearance denominated the “flat disc” by Professor Baer.

In Mammalia a thick and highly transparent membrane,—the true chorion,—is formed external to the proper membrane of the yelk, while the latter is in the ovary. The inner part of the substance of the chorion in its early stages is in a fluid state, so that the yelk-ball moves freely in it; but it subsequently acquires more consistence. There is not any structure corresponding to the chorion in the *ovary* of other vertebrated animals.

The following appears to be the order of formation, as to time, of the more permanent parts of the ovum and the Graafian vesicle in Mammalia, viz. :

1. The germinal vesicle, with its contents, and its envelope of peculiar granules.
2. The proper membrane of the ovisac, which forms around this envelope of granules.
3. The yelk, which forms around the germinal vesicle.
4. The proper membrane of the yelk, which makes its appearance while the yelk is still in an incipient state.
5. The chorion.

6. { The covering or tunic of the ovisac; and about the same time, the peculiar granules of the ovisac arrange themselves to form,
 { The tunica granulosa,
 { The retinacula, and
 { The membrana granulosa.

Such of these structures as are present in the ovary of other Vertebrata, appear to originate in the same order as to time.

"Contributions to the Physiology of Vision." By Charles Wheatstone, Esq., F.R.S., Professor of Experimental Philosophy in King's College, London. *Part the First.* "On some remarkable and hitherto unobserved Phænomena of Binocular Vision."

The author first shows that the perspective projections of an object upon the two retinæ differ according to the distance at which the object is placed before the eyes; if it be placed so distant that to view it the optic axes must be parallel, the two projections are precisely similar; but if it be placed so near that to regard it the optic axes must converge, a different perspective projection is presented to each eye; and these perspectives become more dissimilar as the convergence of the optic axes becomes greater. Notwithstanding this dissimilarity between the two pictures, which is in some cases very great, the object is still seen single; contrary to the very prevalent metaphysical opinion, that the single appearance of objects seen by both eyes is owing to their pictures falling on corresponding points of the two retinæ. After establishing these principles, the author proceeds to ascertain what would result from presenting the two monocular perspectives, drawn on plane surfaces, to the two eyes, so that they shall fall on the same parts of the two retinæ as the projections from the object itself would have fallen. Several means are described by which this may be accomplished; but the author especially recommends for this purpose an apparatus called by him a *stereoscope*, which enables the observer to view the resulting appearances without altering the ordinary adaptation of the eyes, and therefore without subjecting these organs to any strain or fatigue. It consists of two plane mirrors with their backs inclined to each other at an angle of 90° , near the faces of which the two monocular pictures are so placed that their reflected images are seen by the two eyes, one placed before each mirror, in the same place; the apparatus has various adjustments by means of which the magnitude of the images on the retinæ may be varied, and the optic axes differently converged. If the two monocular pictures be thus presented one to each eye, the mind will perceive, from their combined effect, a figure of three dimensions, the exact counterpart of the object from which the pictures were drawn; to show that this curious illusion does not in the least depend on shading or colouring, the illustrations principally employed are simple outline figures, which give for their perceived resultants skeleton forms of three dimensions. Each monocular outline figure is the representation of two dissimilar skeleton forms, one being the form which it is intended to represent, and another, which Prof. Wheatstone calls its converse figure.

Viewed by one eye alone the outline may with equal ease be imagined to be either; but when the two monocular pictures are viewed one by each eye, the proper or the complementary form may be fixed in the mind; the former, if the right and left pictures be presented respectively to the right and left eyes; and the latter, if the right picture be presented to the left eye, and the left picture to the right eye. Many new experiments are then detailed, and a variety of instances of false perception of visual objects, some new, others formerly observed, are traced to these principles; among others, the well-known apparent conversion of cameos into intaglios. The author next proceeds to show that pictures similar in form but differing in magnitude within certain limits, when presented one to each eye, are perceived by the mind to be single and of intermediate size; and also that when totally dissimilar pictures, which cannot be combined by the mind into the resemblance of any accustomed objects, are presented one to each eye, they are in general not seen together, but alternately. The memoir concludes with a review of the various hypotheses which have been advanced to account for our seeing objects single with two eyes; and the author states his views respecting the influence which these newly developed facts are calculated to have on the decision of this much-debated question.

"Experimental Researches in Electricity," *Fourteenth Series*.

On the general nature and relation of the Electric and Magnetic Forces. By Michael Faraday, Esq., D.C.L., F.R.S., &c. &c.

The author commences by observing that the theory of electrical induction, which he had set forth in the 11th, 12th, and 13th series of researches*, does not assume or decide anything as to the real nature of the electric forces, but only as to their distribution; the great question respecting the existence of any electric fluid, or of one, or of two fluids remaining untouched. He then states what the theory does assume; as, for instance, that all *particles*, whether of insulators or conductors, are, as *wholes*, conductors; that, being conductors, they can readily be charged either bodily or polarly; that contiguous particles being on the line of inductive action can communicate their forces more or less readily; that those doing so most readily constitute the bodies called *conductors*, and those doing so least readily those called *insulators*, &c.

Having thus given a brief summary of the conclusions drawn from the previous investigations, the author proceeds to consider the particular condition of the particles which, in an insulating body, are considered as polarized; and after showing that the theory requires that they should be able to polarize in any direction, he states his expectation that a greater facility to polarize in one direction than another would still be found to belong to them, and proceeds experimentally to determine this point. His experiments were made by observing the degree of inductive force across cubes of perfectly crystallized bodies, as rock crystal and

* Prof. Faraday's Eleventh Series of Researches will be found at large, in the present volume, at p. 281 *et seq.*; and abstracts of the Twelfth and Thirteenth Series, in vol. xii., p. 426 and 430.

Iceland spar; these being cut so as to have the axis of the crystal parallel to the line joining two opposite faces of the cube; but the experiments, which are laborious, require extension, and he has not as yet been able to prove or disprove the expected result.

The author then considers whether in compound bodies it is the ultimate and elementary particles or the compound particles which polarize as wholes. He concludes that it is the latter which assume that state; and shows how this point bears upon the electrolyzation of such bodies as are separated into simpler substances, or otherwise altered by the action of the voltaic current.

He then proceeds to certain experiments bearing upon the nature of the relation of the electric and magnetic forces, giving his view of the character of this relation; and concludes his paper by briefly stating what he thinks is more satisfactorily explained by the theory which refers inductive action to an action of contiguous particles than by the old theory.

"Experiments on the Vibration of the Pendulum." By W. J. Frodsham. Communicated by Francis Beaufort, Capt. R.N., F.R.S.

The object of this paper is to show the advantages that may result from attaching to the top of the pendulum a brass tube, which the author terms "an isochronal piece," about five inches in length, fitting the pendulum very nicely, and slit so as to form a spring for about an inch at the bottom, sliding rather stiffly on the rod, so that its position, and consequently its influence on the action of the pendulum, may be varied at pleasure; and that unequal arcs of vibration may be made to correspond to equal intervals of time.

"An Account of some Experiments on the Blood in connexion with the Theory of Respiration." By John Davy, M.D., F.R.S., Assistant Inspector of Army Hospitals.

The author has investigated, experimentally, several of the important questions connected with the theory of respiration and of animal heat; and arrives at the following results. He finds that the blood is capable of absorbing oxygen both from atmospheric air, and from oxygen gas, independently of putrefaction. After blood has been agitated in common air, a trace of carbonic acid, not exceeding one per cent., is found in the residual air; but when pure oxygen is employed, no carbonic acid can be detected in it by the most carefully conducted trials. When pure carbonic acid is brought into contact with blood, or serum, over mercury, and moderately agitated, the absorption of gas exceeds the volume of the fluid. Both arterial and venous blood are rendered very dark, and serum more liquid by the absorption of this gas to saturation. Serum, in its healthy state, is incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and after it has absorbed carbonic acid, only one-tenth of the absorbed gas is expelled by successive agitation with atmospheric air, or with hydrogen. The author is inclined to think that the alkali in the blood, in its healthiest condition, is in the state of a sesquicarbonate. In the majority of trials manifest indications of the disengagement of air from blood *in vacuo* were obtained; but as it occasionally happened

that no air could be thus extricated, the author is induced to believe that the quantity of air contained in the blood is variable: and he has found this air to consist solely of carbonic acid gas. It would also appear, from the experiments detailed in this paper, that a portion of oxygen exists in the blood, not capable of being extracted by the air-pump, yet capable of entering into combination with nitrous gas; and existing in largest proportion in arterial blood. The absorption of oxygen by blood is attended with an increase of temperature.

The experiments of the author tend to show that the lungs are absorbing and secreting, and perhaps also inhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood: and they favour the idea that animal heat is owing, first, to the fixation or condensation of oxygen in the blood in the lungs during its conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

“On the Geometrical Forms of Turbinated and Discoid Shells.” By the Rev. H. Moseley, Professor of Natural Philosophy and Astronomy in King’s College, London. Communicated by Thomas Bell, Esq., F.R.S.

This paper is occupied by an investigation of certain mathematical principles which the author considers as governing the formation of turbinated and discoid shells. According to these views, all such shells may be conceived to be generated by the revolution about a fixed axis of the perimeter of a geometrical figure, which, remaining always similar to itself, increases continually its dimensions. The spiral lines which are observable on the opercula of certain classes of shells, taken in connexion with the well-known properties of the logarithmic or equiangular spiral, appear to have suggested the idea, that not only the boundary of the operculum, which measures the sectional expansion of a shell, but also the spiral lines, which in general are well marked both externally and internally in the shell itself, are curves of this nature.

From an examination of the spirals marked on opercula, it appears that the increase of their substance takes place on one margin only; the other margin still retaining the spiral form, and acquiring an increase of length by successive additions in the direction of the curve. As in the logarithmic spiral the distances of successive spires, measured on the same radius vector produced from the pole, from each other, are respectively in geometrical progression, if similar distances between the successive whorls on the opercula of shells be found to observe the same law, it will follow that these whorls must have a similar form; and that such is the case, the author shows by a variety of numerical results obtained by careful measurements on three different opercula of shells of the order *Turbo*. That such is the law of nature in the formation of this class of shells is rendered probable by the instances adduced by the author, in which a conformity to this law is found to exist.

From the known properties of the logarithmic spiral the author concludes that the law of the geometrical description of turbinated shells is, that they are generated by the revolution about a fixed axis, (namely, the axis of the shell,) of a curve, which continually varies its dimensions according to the law, that each linear increment shall vary as the existing dimensions of the line of which it is the increment. If such be the law of nature, the whorls of the shell, as well as the spires on the operculum, must have the form of the logarithmic spiral; and that this is likewise the case is shown by the almost perfect accordance of numerical results, deduced from the property of that curve, with those deduced from a great variety of careful measurements made of the distances between successive whorls on radii vectores drawn on shells of the *Turbo duplicatus*, *Turbo phasianus*, *Buccinum subulatum*, and in a fine section of a *Nautilus pompilius*. The author further states that, besides the results given in the paper, a great number of measurements were similarly made upon other shells of the genera *Trochus*, *Strombus*, and *Murex*, all confirmatory of the law in question.

One of the interesting deductions which the author has derived from the prevalence of this law in the generation of the shells of a large class of mollusca, is that a distinction may be expected to arise with regard to the growth of land and of aquatic shells, the latter serving both as a habitation and as a float to the animal which forms it; and that, although the facility of varying its position at every period of its growth may remain the same, it is necessary that the enlargement of the capacity of the float should bear a constant ratio to the corresponding increment of its body; a ratio which always assigns a greater amount to the increment of the capacity of the shell than to the corresponding increment of the bulk of the animal.

Another conclusion deducible from the law of formation here considered is, that the growth of the animal, corresponding to a given increment in the angle of the generating curve, will always be proportional to the bulk it has then attained; and if the physical vital energies of the animal be proportional to its actual bulk, its growth, in any given time, will be proportional to its growth up to that time. Hence the whole angle of revolution of the curve generating the shell will be proportional to the whole corresponding time of the animal's growth; and therefore, the whole number of whorls and parts of whorls will, at any period, be proportional to its age.

The form of the molluscous animal remaining always similar to itself, the surface of the organ by which it deposits its shell will vary as the square of the linear dimensions; but as the deposition of its shell must vary as the cube of the same dimensions, there must be an increased functional activity of the organ, varying as the simple linear dimensions.

Since to each species of shell there must correspond a particular number expressing the ratio of the geometrical progression of the similar successive linear dimensions of the whorls; and since the constant angle of the particular logarithmic spiral, which is affected by that species of shell, is deducible from this number, the author

considers that, connected as the form of the shell is with the circumstances of the animal's growth and the manner of its existence, this number, or the angle of the particular spiral, determinable as it is in each case by actual measurement, may be available for the purposes of classification, and may suggest relations by which, eventually, they may become linked with characteristic forms, and modes of molluscous existence.

The concluding portion of the paper contains a mathematical discussion of certain geometrical and mechanical elements of a conchoidal surface. These are, the extent of the surface itself; the volume contained by it; the centre of gravity of the surface, and also of the volume, in each case, when the generating figure revolves about a fixed axis without any other motion, and also when it has, besides this, a motion of translation in the direction of that axis; and, lastly, the angle of the spiral. The author states that his object in this inquiry is the application of these elements to a discussion of the hydraulic theory of shells. The constant angle of the spiral, which each particular species affects, being connected by a necessary relation with the economy of the material of the habitation of each, with its stability, and the condition of its buoyancy, it is therefore necessary to determine the value of this angle.

"On the relative attractions of Sulphuric Acid for water, under particular circumstances; with suggestions of means of improving the ordinary process of manufacturing Sulphuric Acid." By Henry H. Watson, Esq., Corresponding Member of the Manchester Philosophical Society. Communicated by John Dalton, D.C.L., F.R.S., &c.

The object of the inquiry detailed in the present paper is to determine at what degree of concentration the affinity of sulphuric acid for aqueous vapour is equal to that of anhydrous space for the same vapour at given temperatures. It has long been known that concentrated sulphuric acid abstracts moisture from the atmosphere, but the amount and the rate of this absorption have never been ascertained with accuracy; and consequently, in applying this acid to purposes of exsiccation, the experimenter has often been at a loss to know whether the acid was sufficiently strong to render the space in which it was confined perfectly anhydrous. By placing portions of the acid, previously weighed, and diluted with known quantities of water, under the receiver of an air-pump, with equal portions of concentrated acid, of the specific gravity 1.8428, in similar dishes, the author ascertained that the dilute acid could be concentrated to the specific gravity 1.814, at a temperature varying from 65° to 57°: whence he concludes that acid of such strength is capable of drying a vacuum when the temperature does not exceed 57°. By making similar experiments in air, the author compared together the weights lost by ten grains of dilute sulphuric acid of the specific gravity 1.135, at three different periods of the day for six days, taking note of the dew-point and the temperature; and infers that when the affinity of space for vapour, or the evaporating force, is equal to 0.15 of an inch of mercury, it is just able to balance the affinity for water of sulphuric acid of the specific gravity 1.249.

The author next instituted a series of experiments to ascertain whether the evaporation of water from dilute sulphuric acid is capable of being carried on to the same extent in air as in vacuo, and found that the evaporating force of air exerted upon such acid is less than that of a vacuum at the same temperature. He observes that his experiments offer conclusive evidence that the evaporation of water is not owing to the existence of a chemical affinity between the vapour of the liquid and atmospheric air; but thinks that they favour the notion that the obstruction to this process in the open atmosphere is rather owing to the pressure than to the *vis inertiae* of the particles of air. He is also of opinion that improvements will hereafter arise from this inquiry with regard to the economical management of the process of manufacturing sulphuric acid, which process would be greatly expedited by the regulated admission of steam into the condensing chambers kept at a constant high temperature.

The Society then adjourned over the long vacation to meet again on the 15th of November.

LXVI. *Intelligence and Miscellaneous Articles.*

XYLOÏDINE.

THIS name was given by M. Braconnot to a substance produced by the action of concentrated nitric acid upon starch and lignin; the composition of this substance, and of others which accompany its formation have not been examined; and its principal properties have been either imperfectly described or quite unknown. M. Pelouze has lately undertaken the investigation of its properties, and the principal results of his experiments are the following:

If a mixture of starch and nitric acid of sp. gr. 1.5 be made, the starch entirely disappears in a few minutes; the liquor preserves the yellow tint of the concentrated nitric acid, and no elastic fluid is evolved: when immediately treated with water the xyloïdine is entirely precipitated, and the filtered liquor when evaporated yields a scarcely perceptible residue.

If instead of effecting the precipitation by water, immediately after the solution of the starch, the solution is allowed to remain in a close vessel, it becomes gradually coloured, and assumes the various tints of a mixture of nitric acid and nitric oxide. Water then forms a precipitate of xyloïdine, the quantity of which diminishes with time; after two days, and sometimes even after some hours have elapsed, water ceases to render it turbid: the xyloïdine is destroyed and completely converted into a new acid, which is by evaporation procured in the form of a white, solid, uncrystallizable, deliquescent mass, the weight of which is much greater than that of the starch submitted to experiment; but neither carbonic acid nor oxalic acid is produced during this reaction.

The xyloïdine, the first product of nitric acid upon starch, results from the combination of these two bodies; it is common starch containing an atom of water, which is replaced by an atom of nitric acid. The starch is entirely converted into this substance; and this per-

fectly explains the considerable increase of weight which is found when the xyloïdine is precipitated by water immediately after the disappearance of the starch in the nitric acid. As an excess of nitric acid converts the xyloïdine into a very soluble substance, which is the new acid above-mentioned, it explains the different result obtained by M. Braconnot, who procured an equal weight of xyloïdine from a given weight of starch, which according to M. Pelouze was occasioned by the decomposition of a portion of it; by long delaying the precipitation, not the slightest trace of xyloïdine is obtainable.

When the mixture of acid and starch, instead of being allowed to remain by itself, is boiled, the starch is decomposed in a few minutes, and converted into a deliquescent acid, which is obtained pure and in a large quantity by evaporation in a water bath. This acid contains no azote; it has some resemblance to oxalhydric acid (nitrosaccharic acid) but the composition is different. A moderate heat converts it into another acid of a black colour, which is soluble in water, and susceptible of reconversion by nitric acid into the white acid from which it is derived.

Boiling concentrated nitric acid attacks it with the greatest difficulty. When cold it slowly changes it into oxalic acid without producing any carbonic acid. Thus by slow oxidizement, determined by the use of a proper quantity of nitric acid, the starch is successively converted into xyloïdine, deliquescent acid, and oxalic acid, without the carbon suffering the displacement of the other elements of these substances. These curious reactions go on spontaneously in the cold, or in close vessels.

It has been stated that the xyloïdine results from the combination of the starch with the elements of nitric acid. It is a kind of salt in which starch acts the part of base to the nitric acid. It is very combustible; at 236° Fahr. it takes fire and burns vividly, leaving but little residue. This property led the author to an experiment which appears susceptible of several applications, particularly in artillery. When paper is immersed in nitric acid of sp. gr. 1.5, and left there time enough to be penetrated by the acid, or about two or three minutes, on taking it out of the acid and washing it with a great quantity of water, a kind of parchment is obtained, which is impermeable by moisture and extremely combustible: the same happens with cloth made of linen or cotton.—*L'Institut*, October 18th 1838.

DETERMINATION OF IODINE IN KELP.

M. Lassaigne remarks that the extraction of iodine from kelp is now so extensively pursued on account of its numerous applications in medicine and the arts, that by several processes employed in different manufactures, the whole quantity is extracted from commercial products; it cannot therefore, he observes, be unimportant to be able to determine by simple and easy means, the proportion of iodine that different products may furnish.

Several methods may be employed, which are in fact known by all chemists, and often practised by them: thus in precipitating an aqueous solution of kelp, saturated with nitric or sulphuric acid, by ni-

trate of silver, a mixed precipitate of chloride, bromide, and iodide of silver is obtained; the two first are dissolved by ammonia, whilst the iodide remains insoluble; the weight of it then determines that of the iodine contained in the sample.

The new method proposed by M. Lassaigne is founded on the great insolubility of iodide of palladium, which is such that $\frac{1}{400000}$ th of an alkaline iodide is not only detected by the nitrate or chloride of palladium, but the resulting iodide is precipitated in brown flocks, in twelve or sixteen hours, and may be collected on a weighed filter. The weight of the dried iodide indicates that of the alkaline iodide; it is composed, according to M. Lassaigne, of two atoms of iodine and one of palladium.

[In English weights of 1 eq. iodine 126+54, 1 eq. palladium = 180, therefore 180 of the iodide of palladium = 126 iodine.]

The method of proceeding is that of treating a certain quantity of kelp with repeated portions of water, mixing the solutions, saturating with weak nitric acid and adding nitrate or chloride of palladium, when the liquor becomes immediately brown by the iodide of palladium formed. When the quantity is considerable it is readily deposited, but when small, as already noticed, it requires from twelve to sixteen hours to deposit; this reagent is capable of detecting quantities of iodine which are not shown by starch, chloride of platina, or nitrate of silver.—*Journ. de Chem. Med.*, August, 1838.

POLARIZATION OF PLATINA ELECTRODES.

M. Matteucci has communicated the following results of his experiments to the Académie Royale des Sciences.

1. Plates of platina, used as electrodes in water, become covered with traces of oxygen and hydrogen, which they retain for a certain time.

2. Plunged into oxygen or hydrogen, platina plates acquire and preserve for some time a coating of these gases.

3. Plates thus treated and immersed in distilled water develop an electric current moving from the hydrogen to the oxygen plate.

M. Schœnbein also communicated the following remarks on the same subject.

A tube curved in the shape of the letter U and filled with pure hydrochloric acid having a plate of platina plunged into each leg, was placed in communication with the ends of a voltaic battery. After a few minutes the electrodes were removed and fresh plates immersed; a feeble secondary current, detectable by a very delicate galvanometer, was discovered; this passes in a direction opposed to that of the battery. A similar result occurs when dilute sulphuric acid is substituted for hydrochloric acid. From these circumstances M. Schœnbein concludes that electrolytes are capable of becoming *voltaically paralysed* under the influence of a current. He also stated that he does not agree with M. Becquerel in his explanation of the phenomena of polarized plates, because he has observed electrodes to assume the property of polarization when they have conveyed a current of too feeble intensity to decompose iodide of potassium. For

the purpose of explaining this circumstance, M. Schœnbein imagines that although currents of very low intensity may, as Faraday has shown, traverse electrolytes without decomposing them, still he considers that they must produce *some* effect, and this he conceives to be a *weakening* of the bonds of affinity holding together the molecules of the electrolyte; thus in the case of hydrochloric acid the atoms of chlorine and hydrogen become separated to a certain distance from each other, and their mutual affinity diminished, the effect of the current being also to turn the hydrogen molecules towards the negative, and the chlorine towards the positive electrode. In this manner M. Schœnbein explains the secondary current, which on this hypothesis arises from the re-combination of the partially separated atoms of the electrolyte.—*L'Institut*, 252.

[It will be observed that the facts, detailed in the above notice, have for the most part been anticipated by Dr. G. Bird in his paper in the last number of this Journal.—Ed.]

SULPHOCYANIDE OF POTASSIUM AS A TEST FOR STRYCHNIA.

M. Notus having proposed the above-named salt for the use mentioned (*Annals of British Medicine*, Feb. 1837,) M. O. Henry has made the following observations on the subject. He added the sulphocyanide, prepared as directed by M. Notus, to neutral and pure solutions of the acetate of quina, cinchonia, codeia, brucia and strychnia; the three first gave abundant white curdy precipitates, whilst the salt of strychnia yielded silky brilliant crystals, diffused through the liquid. The solutions of acetate of morphia, narcotin, and veratria gave only a slight flocculent cloud, and that of codeia, at first limpid, gave in about a quarter of an hour very fine pearly crystals. All these effects were produced in watch-glasses and were very distinct.

Each precipitate was then gradually heated; those of quina, cinchonia, and codeia disappeared in a short time, but those of brucia, and especially of strychnia, dissolved much more slowly; by gradual cooling they all reappeared with more or less distinct crystalline forms, and the precipitates formed with codeia and strychnia presented their silky appearance.

These precipitates drained and dried on filtering paper were soluble in hot alcohol; with the persalts of iron they produced a crimson colour, like the sulphocyanides; and the bitterness of each was more or less distinguishable.

These results agree with those of M. Notus, and with the characters already known of strychnia: the sulphocyanide of potassium may serve to detect it. It must at the same time be admitted, that several other organic bases give precipitates with it, the properties of which bear great resemblance to those of strychnia; the sulphocyanide of potassium cannot therefore alone be considered as a sufficient indication of this powerful alkali in judicial proceedings, but it adds a new character to those already known.—*Journal de Pharmacie*, xxiv.—194.

ANALYSES OF PECTIC ACID. BY M. V. REGNAULT.

The properties of pectic acid were carefully studied by M. Braconnot, who first extracted it in a pure state from the roots and barks of trees. But the composition of this acid, which acts so important a part in vegetable organization, is entirely unknown to us.

M. Regnault obtained pectic acid nearly by the process given by M. Braconnot.

White turnips freed from their rind were reduced to as fine a pulp as possible, and then washed seven or eight times with rain water. After each washing, the pulp was pressed in small cloth bags. The pulp thus completely lost its smell and taste; it was then boiled for three quarters of an hour with 10 or 12 times its weight of water and a tenth of carbonate of soda; after which it was collected upon a cloth, washed with a small quantity of water, and pressed.

A solution of chloride of calcium was poured into the solution of pectate of soda, which occasioned an abundant precipitation of gelatinous pectate of lime, mixed with much carbonate. This precipitate was for a long time washed upon a cloth, pressed, and then dried in the air. It was this impure pectate of lime which afterwards served for the preparation of the pectic acid and pectates. To separate the pectic acid from it, it was digested for 8 or 10 hours at a temperature of 112° to 140° F. in very dilute hydrochloric acid. The pectic acid set free was collected on a filter and washed with distilled water; it was of a deep colour, and was dissolved in ammonia; the solution was very gelatinous, and could not be filtered, and was put to digest for 24 hours with animal charcoal at a temperature of 140° to 176° F.

By this it became quite colourless, lost its viscosity, and readily passed through the filter. On supersaturating the ammoniacal solution with hydrochloric acid, fresh pectic acid was separated, which was now in the state of a perfectly colourless jelly. This jelly ought to be washed for a long time, first with water acidulated with hydrochloric acid, then with distilled water. It is however rarely thus obtained quite pure; it usually leaves after burning a small residue of earthy matter, which adheres to it with the greatest obstinacy. It is to be redissolved in ammonia or its carbonate, and to be again precipitated from solution. Gelatinous pectic acid cannot be pressed through a cloth, but must be dried in a stove. When dry it is horny, colourless and translucent, extremely hard, and difficult to reduce to powder. It has a slightly acid taste. When digested in hot water it softens, swells a little, but never regains its original gelatinous state. Gelatinous pectic acid is perfectly insoluble in cold water; it appears to dissolve in small quantity in hot water, and especially by long digestion. For if it be boiled for several hours, the filtered solution gives a soluble precipitate with chloride of calcium; but it does not precipitate any thing by mere cooling. When pectic acid is heated, it melts, swells, and leaves a charcoal difficult to burn.

Dilute nitric or hydrochloric acid, even when heated, does not act upon it; strong nitric acid attacks it rapidly, and oxalic and mucic acid are formed. Cold concentrated sulphuric acid does not appear to act upon it, but when heated decomposes it readily.

Caustic potash at a temperature above 392° F. acts strongly on pectic acid, and Gay-Lussac has remarked that much oxalic acid is formed. Pectic acid dried at 284° F. yielded by analysis

| | |
|---------------|------------|
| Hydrogen..... | 4.71 |
| Carbon..... | 43.21 |
| Oxygen | 52.08—100. |

Journal de Pharmacie, 24, 201.

PECTATES OF POTASH, SODA, AND AMMONIA.

M. Regnault states that pectic acid combines very perfectly with bases, and in general forms gelatinous compounds. The pectates of potash, soda and ammonia are alone soluble in water; all others are insoluble and form transparent jellies, more bulky and firm than the acid itself, and they may be pressed through a cloth when they have been precipitated from cold solutions; when precipitated hot they are not firm, but are like the acid itself.

It is extremely difficult, and perhaps even impossible, to obtain the pectates in a state of determinate saturation; all efforts to obtain them failed. This difficulty is derived from the impossibility of always procuring definite soluble pectates.

If a solution of potash be digested with an excess of gelatinous pectic acid, it dissolves in large quantity, the liquor loses its alkaline reaction, and after a certain time, it even becomes slightly acid, evidently owing to an excess of the acid. If alcohol be added to a solution of pectate of potash or soda, a gelatinous precipitate is formed, which appears to be a sub-pectate. This jelly when washed with alcohol loses alkali, and probably becomes neutral pectate, when the washing is sufficient; but it is difficult to determine the exact point.

Neutral pectate of ammonia is not obtained more readily; if pectic acid be dissolved in excess of ammonia, a solution is obtained, which by long boiling becomes acid to test paper; this when evaporated in vacuo leaves a transparent mass, which when dissolved in water has also an acid reaction; in both these cases, we obtain a pectate with excess of acid. The insoluble pectates leave very hard horny masses, and which it is extremely difficult to powder; it is therefore also very difficult to burn them with oxide of copper. To obtain the combustion perfectly, it is necessary to put a certain quantity of chlorate of potash mixed with oxide of copper at the bottom of the tube.

When heated in contact with the air, dry pectates take fire, and continue to burn till all the organic matter is destroyed. The pectates retain the last portions of water with considerable force; they must be heated to 284° or 302° F. to render them perfectly dry; they do not begin to decompose till heated to about 392°; the dry pectates are very hygrometric; when they are to be analysed they must be quickly weighed, and dried afterwards in the combustion tube, and kept at a temperature near 212°.—*Ibid.*

ON THE DECOMPOSITION OF SILICEOUS MINERALS BY MEANS OF HYDROFLUORIC ACID.

The reason this method of decomposing siliceous substances, first pointed out by Berzelius, is so seldom adopted, no doubt arises from the necessity of preparing pure hydrofluoric acid in platina retorts, with which few chemists are provided. In order to overcome this difficulty, Mr. C. Brunner has proposed to place the siliceous substance to be acted upon, in an atmosphere of hydrofluoric acid, which for this purpose he prepares in a leaden vessel of this form: the diameter is about 6 inches, and the other dimensions in proportion. In the middle of this vessel, supported by leaden feet, a very flat platina dish is placed, in



which is spread the siliceous substance reduced to the finest powder and moistened with a little water. The bottom of the leaden vessel is covered to the depth of $\frac{1}{4}$ to $\frac{1}{8}$ of an inch with a mixture made to the consistence of a paste, of fluat of lime, and sulphuric acid. A small flat cover with a wooden handle closes the vessel. So provided it is submitted to the gentle heat either of a sand-bath or small lamp. By means of the hydrofluoric acid gas thus given out a quantity from 20 to 30 grains of siliceous mineral is completely decomposed in the space of one or two hours. During the progress the powder must be twice moistened with a few drops of water. If the powder is well spread out at first, it is seldom necessary to stir it. When this operation is finished a quantity of concentrated sulphuric acid is added, drop by drop, to the powder as long as any hydroflu-silicic acid is given off, at the same time it is to be warmed by a spirit-lamp, and finally the excess of sulphuric acid is driven off by a continued heat and evaporation to dryness. The dry residue after being moistened with muriatic acid is boiled in water, and further examined in the usual way.

This method is particularly applicable to the examination of such minerals as contain alkalis.—*Poggendorff's Annals*, No. v. 1838, p. 134.

ON THE SEPARATION OF COMPOUNDS OF OXIDES OF ANTIMONY AND LEAD.

During some experiments on the decomposition and preparation of a colour known in commerce by the name of Naples yellow, Mr. C. Brunner found the greatest difficulty in separating this compound. Upon most of the samples, acids had very little or no action, nor did heating with alkalis effect his purpose. He then thought of converting them into sulphurets in the following manner, which completely succeeded. About 30 grains were carefully mixed with 75 grains of sulphur and 150 grains of recently heated carbonate of potash, and the whole melted in a glass retort. As soon as it was uniformly liquid and the dark brown mass had cooled the retort was filled with water. The sulphuretted solution contained all the antimony and the undissolved residue contained all the

lead as a sulphuret (Pb S.). The latter was dried and the quantity of lead calculated. The solution, containing the antimony in the state of a sulphuret, was carefully neutralized by dilute muriatic or acetic acid, and the precipitate collected upon a weighed filter, dried, weighed, and a portion of it reduced to the metallic state by means of a stream of dry hydrogen gas, as described by Rose, *Analytical Chemistry*, vol. ii. After the quantity of each metal was thus ascertained, the state of the oxidation of the antimony was still required. For this purpose the lead found was calculated as an oxide. This weight added to the weight of the antimony, deducted from the weight of the sample examined, gives the quantity of oxygen combined with the antimony.

The experiments which were performed upon several different samples of this colour as usually found in commerce, proved it to be a compound of oxide of lead with antimonious acid (Antimon saure). But one sample received from Paris of great beauty turned out to be a mixture of white lead and sulphuret of cadmium.—*Poggendorff's Annals*, No. V. 1838, p. 135.

EXTRACT FROM A LETTER FROM PROFESSOR JOHNSTON ON
THE ANALYSIS OF THE RESINS.

Durham, Oct. 27, 1838.

MY DEAR PHILLIPS,—I am desirous of acquainting you with the partial results of an investigation in which I am occupied, and which I think will prove very interesting. I have been analysing some of the resins, and so far as I have gone, I think they may all be represented by a modification of the formula $C_{40}H_{32}$, that for oil of turpentine, the carbon being constant and the hydrogen variable. You are aware, probably, that the three resins hitherto analysed agree with this view, thus:

| | | |
|--------------------|---|---|
| Colophony | = | $\left\{ \begin{array}{l} C_{40}H_{32}O_4. \text{ Rose.} \\ C_{40}H_{30}O_4. \text{ Liebig and Laurent.} \end{array} \right.$ |
| Crystallized elemi | | $C_{40}H_{32}O_2. \text{ Rose.}$ |
| Crystallized animi | | $C_{40}H_{33}O. \text{ Laurent.}$ |

Circumstances have led me to examine various other resins, with the following results:

| | |
|----------------|---|
| Middletonite | $C_{40}H_{22}O_2$ |
| Gamboge | $C_{40}H_{24}O_3 ?$ |
| Guyaquillite | $C_{40}H_{26}O_6$ |
| E. India Animi | $\left. \begin{array}{l} \\ \end{array} \right\} C_{40}H_{31}O_3$ |
| Copal B | |
| Highgate resin | $C_{40}H_{32}O$ |
| Mastic | $\left. \begin{array}{l} \\ \end{array} \right\} C_{40}H_{32}O_4$ |
| Copal A | |

The sparingly soluble part of copal is B. That which easily dissolves is the same as the easily soluble part of mastic. The small difficultly soluble part of the latter differs from copal B, at least in properties.

Berengelite $C_{40}H_{31}O_8$
General formula = $C_{40}H_{32} \pm x Oy$.

This last resin is one said to occur abundantly in a kind of pitch

lake at St. Juan de Berengela in South America. You will be struck by the analogy between the Highgate resin and the animi of Laurent; they are probably the same. I have not been able to get any animi having that composition: various resins seem to find their way into the market under that name.

ANALYSIS OF COMPTONITE.

On account of the rarity of comptonite it has not yet been analysed, and mineralogists have been uncertain what place it ought to occupy; it is found in the lava of Vesuvius, and in the basalt near Eisenach in Saxony, and also in Bohemia. A specimen from Ellenbogen in Bohemia, analysed by M. E. Melly, gave

| | | | | | | |
|------------------------|---|---|---|---|---|-----------|
| Silica | - | - | - | - | - | 37 |
| Alumina | - | - | - | - | - | 31.07 |
| Lime | - | - | - | - | - | 12.60 |
| Soda | - | - | - | - | - | 6.25 |
| Water | - | - | - | - | - | 12.24 |
| Trace of iron and loss | - | - | - | - | - | 0.84—100. |

It is therefore a hydrous silicate of alumina, lime and soda.—*L'Institut*, 27th Sept. 1838.

ACTION OF CHLORINE ON ACETIC ACID.

M. Dumas states that he had long since obtained, by the action of solar influence on dry chlorine and crystallizable acetic acid, a chlorated, volatile, crystallizable acid, with other products which were separated with difficulty. On dissolving the acid in water and distilling the dry residue on anhydrous phosphoric acid, the new acid was obtained in a pure state. Its analysis gave the following formula: $C^8 H^2 C^6 O^4$; the analysis of this acid was verified by that of its alcoholic and methylic æthers, and also by its salt of lime.—*L'Institut*, 30th Aug. 1838.

ACTION OF CHLORIDE OF ZINC ON ALCOHOL.

M. Masson, Professor of Physics at the Royal College of Caen, distilled a mixture of alcohol and chloride of zinc at a temperature of about 322° Fahr., by this he obtained an oil, the production of which did not cease till the temperature reached 392°. He compared it with the sweet oil of wine, and observed that it was separable into two products of unequal volatility; the less volatile boiled at about 572°, when freed from all extraneous matter. It possessed all the characters of the light sweet oil. By analysis it yielded,

| | | | | | | |
|----------|---|---|---|---|---|------------|
| Hydrogen | - | - | - | - | - | 12.8 |
| Carbon | - | - | - | - | - | 88.1—100.9 |

which corresponds to $H^7 C^8$.

The most volatile product boiled below 212°. It is very fluid. Its smell resembled that of naphtha. Its composition is remarkable, for it is the most hydrogenated of the liquid carburetted hydrogens. It consists of

| | | | | | | |
|----------|---|---|---|---|---|-------------|
| Hydrogen | - | - | - | - | - | 15.7 |
| Carbon | - | - | - | - | - | 84.5—100.2. |

From which its formula appears to be $H^9 C^9$; but according to the density of its vapour which is equal to 4.18 it may be represented by $H^{36} C^{32}$.

There are therefore in the oil in question two carburets of hydrogen, on adding which together the carburetted hydrogen from which they are derived is reproduced.—*L'Institut*, 30th Aug. 1838.

COMBINATION OF AZOTE WITH METALS.

The experiments of Thenard and Despretz seem to have proved that certain metals absorb azote at a high temperature; it is, however, observed by M. Pfaff, that Berzelius has remarked that the experiments of Despretz are incomplete: and Pfaff states that it appeared to him that a different mode from that followed by Despretz might be adopted, to prove the absorption of azote by certain metals at a red heat. It is well known that ammoniacal gas when resolved into its elements, is converted into a mixture of three volumes of hydrogen and one volume of azote. In the decomposition of ammoniacal gas, as effected during its passage over red-hot metals, the same relation ought to exist between the volumes of hydrogen and azote, if the azote is not absorbed by them; and if the remarkable alteration which their properties undergo does not depend on the absorption of this gas, there ought, on the other case, to be an increase in the proportion of the azote. In order to perform these experiments, M. Pfaff selected good porcelain tubes, in which was placed a copper wire, two lines in diameter, and of considerable length. The ammoniacal gas was disengaged from sal ammoniac by lime, and conducted over caustic potash, exposed to a strong red heat; the other end of the porcelain tube was made to communicate, by a tube bent at a right angle, with a Woulf's bottle containing water to absorb the undecomposed ammoniacal gas. The experiment was continued several hours, to decompose a great quantity of ammoniacal gas. The gas was received in separate portions; at the close of the experiment the copper wire exhibited beautiful iridescence, and those parts of it which had suffered most change were extremely friable. The gas disengaged was examined in a Volta's eudiometer.

The following were the results of the examinations of the portions of gas, in the order in which they were received:

First portion.—400 volumes were detonated with 200 volumes of oxygen gas; the residue was 200 volumes, consequently 400 volumes had disappeared, of which $266\frac{2}{3}$ were hydrogen; so that the 400 volumes of the gas analysed contained only $266\frac{2}{3}$ of hydrogen gas. From the following experiments it may be concluded that this first portion was mixed with a portion of the atmospheric air of the vessels.

Second portion.—300 volumes of this gas were detonated with 300 volumes of oxygen gas; the residue was 213, there had consequently disappeared 387, of which 258 were hydrogen. These 300 volumes of gas decomposed, if they were composed of hydrogen and azote in the proportion of 3 : 1, ought to contain only 225 volumes of hydrogen and 75 of azote; there was, therefore, an excess of 33 in 300, and the azote amounted to only about one-seventh, instead of one-fourth.

Third portion.—300 volumes were detonated with 300 of oxygen

gas; the residue was 210 volumes; 390 had therefore disappeared. Of this number, 260 were hydrogen, which indicates a loss of azote nearly equal to that of the second portion.

As the excess of hydrosulphur cannot be attributed to the decomposition of the vapour of water, since it is well known that copper does not decompose water at a red heat, and as the requisite means for drying the ammoniacal gas were adopted, the only explanation of the facts which can be given, is that a strong red heat sufficiently increases the affinity of copper for azote to cause their combination, and consequently it is owing to this that the physical properties of the copper are altered, as first remarked by Thenard and confirmed by Despretz.—*Journal de Pharmacie*, Aug. 1838.

SOLUBILITY OF BINOXIDE OF MERCURY IN WATER.

The solubility of binoxide of mercury, which has been stated to exist by some chemists, has been objected to by others, because the oxide employed might be supposed to contain some nitrate of mercury. To obviate this objection, M. Boudet employed some *precipitate per se*, which was prepared by Deyeux, and had been kept in the matras in which it was prepared. A portion of this oxide was triturated in a mortar to an impalpable powder, was rubbed with distilled water at 50° Fahr.; when filtered, it rendered a weak solution of syrup of violets decidedly green; but hydrosulphate of ammonia produced no effect. On the contrary, boiling water took up so much oxide, that it made syrup of violets of a decided green colour, and restored the colour of reddened litmus; hydrosulphate of ammonia rendered the solution brown, and eventually flocks of a blackish precipitate were deposited.—*Journal de Pharmacie*, tome xxiv. 253.

DECOMPOSITION OF LITHIC ACID BY NITRIC ACID.

MM. Wœhler and Liebig have found that by decomposing lithic acid by nitric acid, two new compounds are formed, which they observe have no analogy in chemistry. They both crystallize, but one is very soluble and the other slightly so; reducing the equivalents to the English standard, the *soluble* is composed of 5 eqs. of hydrogen, 8 of carbon, 10 of oxygen, and 2 of azote; while the slightly soluble contains one equivalent less of hydrogen. One of these products is easily converted into the other; by heating the soluble one with nitric acid, one equivalent of hydrogen is removed, and the less soluble is obtained. When, on the other hand, the less soluble is treated with hydrosulphuric acid, sulphur is deposited and hydrogen added, so as to reproduce the compound containing 5 eqs. of hydrogen.

These are the two substances which, when they are mixed with ammonia, produce what is called purpurate of ammonia, one of the most brilliant preparations of organic chemistry. Separately taken neither furnishes the purpurate of ammonia. The composition of this substance is then extremely complicated; it is a new kind of *amide*. The authors have been able to arrive at a complete and satisfactory explanation of its production.

It is impossible not to be struck with the analogy of the soluble compound with *orcein*, and that of purpurate of ammonia with *orcein*. By heating orcein with dilute nitric acid, and adding ammonia, the solution assumes a very deep red colour, which, however, is never so fine as that of the orcein of M. Robiquet.—*L'Institut*, March 1838.

LACTATE OF UREA.

MM. Cap and Henri by treating lactate of lime with oxalate of urea obtained lactate of urea, which crystallizes in very white prismatic needles, and possesses very distinct chemical characters. This lactate has also been found by them in a natural state; after having separated the free lactic acid from urine by an excess of hydrate of zinc, they obtained this salt in crystals, and perfectly identical with that prepared by direct combination.—*L'Institut*, March 1838.

CAOUTCHOUC IN PLANTS.*

The substance caoutchouc is a widely disseminated constituent of vegetable fluids. It has hitherto, I believe, been found only in plants with milky juice, although its presence in all plants yielding such fluid remains to be proved. The presence of caoutchouc in silk has been, I believe, attributed to the nature of the fluids of the plants on which the caterpillars feed; but this, although applicable to the mulberry plants, can scarcely hold good with the various species of *Tetranthera* on which the Moonga feeds, or with the castor-oil plant, the chief food of the Eria, which in Assam does not appear to yield milk. Milky juice is often characteristic of certain families, but often not; its presence is frequently of importance, as it often affords valuable indications of affinity. It is remarkable that it is almost unknown in the grand division of Monocotyledonous plants. The families in which its presence may be said to be universal are *Apocqueea*, *Asclepiadea*, *Campanulacea*, *Sobeliacea* and the great division of *Compositæ*, *Chicoracea*, of which the lettuce is a familiar example. It is of common occurrence in *Euphorbiacea* and *Tulicea*, which orders may be looked on as the grand sources of caoutchouc. Thus, in addition to our Indian plants, the American caoutchouc is supposed to be produced by *Cecropia peltata*, which belongs to *Urticea*, and the ule tree of Papantla, from which the caoutchouc of that country is obtained, is supposed to belong to the same orders. I must, however, observe that Baron Humboldt objects to the supposition of *Cecropia peltata* yielding the American caoutchouc, as its juice is difficult to inspissate†.

The order *Euphorbiacea* would likewise appear to supply a large quantity. Thus Dr. Lindley informs us that the true caoutchouc is furnished by *Siphonia elastica*, *Hevia quiancensis* of Aublet, a Surinam and Brazilian tree; and it is from a tree of this order that a substance resembling caoutchouc is procured in Sierra Leone.

Some *Apocqueea* are also reported to produce good caoutchouc‡; thus *Aricola elastica* produces the caoutchouc of Sumatra, and it is

* From Mr. Wm. Griffith's Report. Journ. of the Asiatic Soc. of Bengal.

† Lindley's Introduction to Natural System of Botany, p. 176.

‡ Lindley's Instructions, p. 300.

from this plant that caoutchouc has been produced in Penang and exported to England*. *Willughbeia edulis* is likewise an Indian plant from which caoutchouc has been produced, but Roxburgh says it is of indifferent quality: unless I have been misled, good caoutchouc is obtained from *Nerium grandiflorum* of Roxburgh.

It is probably equally abundant in *Asclepiadea*; one plant of which order *Cynanchum albiflorum* has been stated to yield it of excellent quality in Penang. Mr. Royle seems inclined to attribute the great tenacity of the fibres of some plants of both these orders to its presence, but this supposition seems to me of very doubtful accuracy †.

NEW ANOMALOUS REPTILE.

Dr. John Natterer, the industrious collector, who has lately returned to Vienna from South America with his large collections, has published in the 'Annals of the Vienna Museum' (ii. p. 167.) under the name of *Lepidosiren paradoxa*, a new anomalous reptile, which has much the appearance of an eel, but is covered with large netted scales, and the body is furnished with four simple elongated tapering legs; the front pair being placed on the back edge of the upper part of the spiracles, and the hinder pair on the under side of the hinder part of the body. The jaws are furnished with strong truncated teeth, and the vent, which is circular and plaited, is placed on the left of the centre of the under side of the body, just behind the base of the left hind leg. It was discovered in the Brazils near the river Amazon, and grows to three feet in length. They had two specimens in the Vienna Museum; one of them has been put into the hands of Professor Th. Bischoff for the purpose of being dissected.—*Ann. of Nat. Hist.* No. X.

METEOROLOGICAL OBSERVATIONS FOR OCTOBER 1838.

Chiswick.—Oct. 1. Hazy. 2. Cloudy: fine. 3. Fine. 4. Very fine. 5. Hazy: fine. 6, 7. Overcast. 8, 9. Bleak and cold. 10. Cloudy. 11. Cloudy and fine. 12. Clear and cold: slight snow. 13. Clear: showery: frosty at night. 14. Overcast: showery: frosty at night. 15, 16. Cloudy. 17, 18. Fine. 19. Overcast: clear and fine. 20. Rain: overcast. 21—24. Cloudy and fine. 25. Foggy: clear at night. 26. Fine. 27. Cloudy: stormy and wet at night. 28. Hazy: heavy rain: violent hurricane from s.w. during the night. 29. Clear and windy. 30. Fine. 31. Heavy rain.

Boston.—Oct. 1. Cloudy. 2, 3. Fine. 4. Cloudy. 5. Fine. 6, 7. Cloudy. 8. Cloudy: rain p.m. 9—11. Cloudy. 12. Stormy: snow p.m. 13. Fine: ice this morning one-eighth of an inch thick. 14. Cloudy: rain p.m. 15. Cloudy. 16. Cloudy: stormy p.m. 17, 18. Fine: stormy p.m. 19—21. Fine. 22, 23. Cloudy. 24. Cloudy: rain early a.m. 25. Cloudy. 26. Cloudy: rain early a.m. 27. Fine: stormy with rain p.m. 28. Fine: rain p.m. 29. Stormy: rain early a.m. 30. Fine. 31. Cloudy.

Appletharpe Manse, Dumfries-shire.—Oct. 1, 2. Fine harvest days. 3. Ditto, but cloudy: frosty p.m. 4, 5. Very warm: frost rime. 6. Warm: not a breath of wind. 7. Mild day, but cloudy. 8. Fine harvest day. 9. Ditto, but threatening rain. 10. Still lowering and threatening rain. 11. High wind: slight shower. 12. Dry and cold. 13. Ditto: snow showers passing. 14. Wet all day. 15. Moist and stormy. 16. Wet all day. 17. Drying day. 18. Wet all day. 19. Drying again. 20. Clear and drying. 21, 22. Moist: showers p.m. 23. Fair a.m.: came on rain. 24. Showery. 25. Very wet p.m. 26. Rivers in flood: drying p.m. 27. Showery all day: flood again. 28. Hoar frost a.m.: fine p.m. 29. Partial showers. 30. Fine day: slight rains. 31. Fair: wet afternoon.

* Royle's Illustrations, p. 329, under *Euphorbiaceae*, and p. 270, under *Anacardium*.
† Royle's Illustrations, p. 274.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTSON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VALL at Boston, and by Mr. DUNBAR at Applegarth Manse, Dumfriesshire.

| Days of Month.
1888.
Oct. | Barometer. | | | | Thermometer. | | | | Wind. | | | | Rain. | | | Dew-point. | | | | | | |
|---------------------------------|-----------------------------|-----------|--------|--------------------|---------------------------|-------------------------|-------------------|------------------------|-------------------|------|---------------------------|-----------------------------|---------------------|---------------------------|-----------------|---------------------------|-----------------------------|---------------------|-------------------|---------------------------|----------------------|------|
| | London: Roy. Soc.
9 a.m. | Chiswick. | | Boston.
8½ a.m. | Dumfries-shire.
9 a.m. | London: Fahr.
9 a.m. | Roy. Soc.
Min. | Self-register.
Max. | Chiswick.
Max. | Min. | Dumfries-shire.
9 a.m. | London: Roy. Soc.
9 a.m. | Chiswick.
9 a.m. | Dumfries-shire.
9 a.m. | Boat.
9 a.m. | Dumfries-shire.
9 a.m. | London: Roy. Soc.
9 a.m. | Chiswick.
9 a.m. | Boston.
9 a.m. | Dumfries-shire.
9 a.m. | Dew-point.
9 a.m. | |
| | | Max. | Min. | | | | | | | | | | | | | | | | | | | |
| 1. | 30.338 | 30.357 | 30.318 | 29.83 | 30.42 | 30.44 | 51.8 | 60.6 | 52.6 | 59 | 51 | NW. | N. | 49½ | E. | 54 | NW. | N. | 0.1 | ... | 52 | |
| 2. | 30.356 | 30.437 | 30.348 | 29.90 | 30.52 | 30.58 | 55.4 | 57.6 | 52.2 | 63 | 47 | N. | N. | 48 | E. | 48 | N. | N. | ... | ... | 52 | |
| 3. | 30.428 | 30.444 | 30.406 | 30. | 30.57 | 30.50 | 55.7 | 60.4 | 47.8 | 64 | 42 | NE. | NE. | 44 | E. | 44 | NE. | NE. | ... | ... | 50 | |
| 4. | 30.394 | 30.395 | 30.376 | 29.93 | 30.43 | 30.40 | 51.0 | 61.2 | 53.2 | 61 | 37 | NE. | NE. | 42 | calm | 42 | NE. | NE. | ... | ... | 48 | |
| 5. | 30.352 | 30.406 | 30.338 | 29.90 | 30.37 | 30.37 | 49.7 | 58.8 | 46.6 | 59 | 46 | N. | E. | 44 | calm | 44 | E. | calm | ... | 0.02 | 48 | |
| 6. | 30.372 | 30.410 | 30.379 | 29.92 | 30.37 | 30.41 | 49.8 | 56.6 | 46.9 | 56 | 49 | NW. | NE. | 50 | NE. | 50 | NW. | NE. | ... | ... | 45 | |
| 7. | 30.372 | 30.385 | 30.343 | 29.93 | 30.40 | 30.53 | 51.4 | 54.5 | 49.2 | 56 | 47 | NW. | NE. | 46½ | calm | 46½ | NW. | NE. | ... | ... | 47 | |
| 8. | 30.352 | 30.412 | 30.351 | 29.92 | 30.38 | 30.38 | 52.0 | 54.9 | 48.8 | 56 | 47 | NW. | NE. | 42 | calm | 42 | NW. | NE. | ... | ... | 47 | |
| 9. | 30.360 | 30.367 | 30.366 | 29.89 | 30.33 | 30.23 | 52.4 | 55.7 | 49.0 | 56 | 47 | NW. | NE. | 50 | calm | 50 | NW. | NE. | ... | ... | 48 | |
| 10. | 30.230 | 30.234 | 30.159 | 29.74 | 30.10 | 30.00 | 51.3 | 57.6 | 50.0 | 57 | 48 | NW. | NW. | 46 | calm | 46 | NW. | NW. | ... | 0.7 | 47 | |
| 11. | 29.996 | 30.100 | 29.859 | 29.47 | 29.75 | 29.39 | 52.8 | 54.3 | 49.9 | 60 | 49 | SW. | NW. | 52½ | calm | 52½ | NW. | NW. | ... | ... | 47 | |
| 12. | 29.652 | 29.758 | 29.657 | 29.21 | 29.59 | 29.64 | 46.2 | 59.7 | 42.8 | 47 | 42 | W. | NW. | 34 | calm | 34 | SW. | SW. | ... | ... | 48 | |
| 13. | 29.762 | 29.975 | 29.806 | 29.40 | 29.78 | 29.82 | 39.4 | 50.3 | 36.4 | 44 | 28 | W. | NW. | 38 | N. | 38 | N. | N. | ... | 0.30 | 44 | |
| 14. | 29.904 | 29.929 | 29.497 | 29.44 | 29.55 | 29.28 | 41.4 | 45.0 | 32.8 | 49 | 26 | W. | NW. | 48 | N. | 48 | N. | N. | ... | ... | 39 | |
| 15. | 29.574 | 29.656 | 29.616 | 29.08 | 29.36 | 29.30 | 51.8 | 53.8 | 40.4 | 56 | 47 | W. | NW. | 52½ | N. | 52½ | NW. | NW. | ... | ... | 48 | |
| 16. | 29.614 | 29.684 | 29.442 | 29.04 | 29.29 | 29.04 | 55.7 | 59.7 | 48.8 | 60 | 51 | SW. | SW. | 52 | W. | 52 | SW. | SW. | ... | ... | 43 | |
| 17. | 29.292 | 29.548 | 29.293 | 28.76 | 29.02 | 29.49 | 54.5 | 61.0 | 53.5 | 55 | 37 | SW. | SW. | 49½ | NW. | 49½ | NW. | NW. | ... | ... | 50 | |
| 18. | 30.054 | 30.013 | 29.861 | 29.44 | 29.77 | 29.39 | 46.2 | 59.3 | 42.0 | 54 | 42 | SW. | SW. | 48 | NW. | 48 | SW. | NW. | ... | ... | 41 | |
| 19. | 29.910 | 30.073 | 29.833 | 29.42 | 29.74 | 29.80 | 57.8 | 58.4 | 46.2 | 62 | 45 | SW. | SW. | 48 | NW. | 48 | SW. | NW. | ... | ... | 50 | |
| 20. | 30.088 | 30.199 | 29.923 | 29.50 | 29.82 | 30.04 | 57.8 | 59.3 | 54.0 | 66 | 41 | SW. | W. | 48 | NW. | 48 | SW. | NW. | ... | ... | 51 | |
| 21. | 30.296 | 30.250 | 29.981 | 29.64 | 30.03 | 30.03 | 55.8 | 63.5 | 48.4 | 64 | 54 | SE. | SW. | 52 | calm | 52 | SE. | SW. | ... | ... | 52 | |
| 22. | 30.156 | 30.116 | 30.043 | 29.59 | 29.86 | 29.74 | 57.4 | 61.2 | 55.7 | 61 | 53 | SE. | S. | 52½ | calm | 52½ | S. | calm | ... | ... | 52 | |
| 23. | 29.960 | 29.924 | 29.765 | 29.42 | 29.70 | 29.51 | 56.7 | 61.0 | 54.8 | 57 | 52 | SE. | S. | 52 | calm | 52 | S. | calm | ... | ... | 53 | |
| 24. | 29.740 | 29.928 | 29.742 | 29.21 | 29.50 | 29.85 | 55.0 | 58.6 | 53.7 | 63 | 39 | S. | W. | 48 | calm | 48 | S. | calm | ... | ... | 52 | |
| 25. | 30.118 | 30.056 | 29.947 | 29.20 | 29.90 | 29.50 | 50.7 | 59.9 | 46.0 | 58 | 48 | SE. | SE. | 50½ | W. | 50½ | SE. | W. | ... | ... | 50 | |
| 26. | 29.780 | 29.925 | 29.795 | 29.19 | 29.43 | 29.74 | 57.3 | 58.2 | 49.0 | 62 | 30 | SW. | W. | 42 | W. | 42 | SW. | W. | ... | ... | 52 | |
| 27. | 29.922 | 29.890 | 29.755 | 29.45 | 29.43 | 29.33 | 49.7 | 59.3 | 41.5 | 57 | 47 | S. | SW. | 46 | calm | 46 | S. | SW. | ... | 2.90 | 47 | |
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